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SCIENCE AND THE PRESS

by

WILLIAM E. DICK

Mr. Dick, a biologist and Fellow of the Linnean Society, has been editor of Discovery, *the well-known British science magazine, for the past 10 years.*

In this article the word 'press' is used in its broadest sense, the sense which attaches to it in the expression 'the freedom of the press'; it thus covers all printed matter that is freely published. It would of course have been possible to have confined the article to the relationships of science and the press in the narrow modern sense of mass circulation newspapers and magazines; but this approach was rejected as being historically a bad one in that it would have eliminated one important consideration, namely the part which the printed word played in the development of science itself. By rejecting the narrower treatment, it is also possible to consider the extent to which scientists and professional writers have common interests, for example, in such matters as the freedom to publish. This article may therefore have something more than a merely ephemeral value; and certain scientists and writers of the past would have agreed with the main burden of its argument, in particular the first secretary of the Smithsonian Institution, Joseph Henry, who wrote that the 'principal means of diffusing knowledge must be the *Press*'.

THE FIRST SCIENTIFIC JOURNALS

One look round any laboratory today shows that scientific publications, and especially scientific periodicals, are quite indispensable tools. The first scientific periodicals came into existence in the seventeenth century, very soon after the foundation of the first scientific societies.[1] In many ways these journals were even more important for the diffusion of new scientific knowledge than the actual meetings of the societies. One of the first was the *Philosophical Transactions,* the first number of which appeared in 1665.[2]

1. For example, the Accademia dei Lincei in Rome (*c.* 1601), the Royal Society of London (1662), the Academy of Sciences in Paris (1666), the Berlin Academy (1700) and the St. Petersburg Academy (1724).

2. In 1665 the Royal Society's Council passed a resolution to the effect that 'the Philosophical Transactions, to be composed by Mr. Oldenburg [the society's secretary], be printed on the first Monday of every month, if he have sufficient matter for it, and that the tract be licensed under the Charter by the Council of the Society, being first reviewed by some members of the same; and that the President be now desired to license the first papers thereof, being written in four sheets in folio'. Oldenburg remained editor until his death in 1677; afterwards his three immediate successors as editor of the *Philosophical Transactions* were Hooke, Dr. Robert Plot and Halley.

This journal typified the periodicals that were established by scientific societies of that time, and it quickly established itself as 'a convenient Register for the bringing in and preserving of many experiments, which, not enough for a book, would else be lost, and have proved a very good ferment for the setting of uncommon thoughts in all parts a-work'. Actually the *Philosophical Transactions* started as an unofficial journal 'not to be looked upon as the business of the Royal Society'; but the scientific world generally came to accept it as an official publication of the Royal Society's Council, and eventually the society had to accept this *fait accompli,* making the journal its official publication in 1753. Thus we find that the *Philosophical Transactions* is a more ancient periodical than the *London Gazette,* which dates back to 1666. So far as I can discover it is the oldest British periodical still in existence.

Paris had seen the publication of an even earlier journal which carried accounts of scientific progress; this was the *Gazette de France* (founded 1631), which printed reports of small private scientific conferences held '*tous les lundis chez son fondateur*', who was Renaudot (1586-1653). This was not however an exclusively scientific journal. In 1664 Denis de Sallo (1619-83) brought out his *Journal des Sçavans,* which devoted much space to scientific subjects as well as publishing literary and philosophical contributions. The early members of the Royal Society were familiar with this journal, and from the temporary suppression of the *Journal des Sçavans* in March 1665 they learnt the wisdom of not interfering 'with legal or theological matters'; Oldenburg planned the *Philosophical Transactions* as something 'much more philosophical' than de Sallo's journal, and from the start it was designed to publish 'philosophical matters which come from abroad', together with accounts of experiments—'at least the most important'—performed in England. Both in Britain and France the press was strictly controlled, and it is not hard to imagine how the scientists of the period felt about their new-fledged journals: rather than jeopardize the existence of these periodicals they were prepared to err on the side of caution, with the result that it was left to the literary men and professional writers to fight out the battle for the freedom of the press, men such as Milton, Voltaire and Diderot, and the rising ranks of journalists.

In Germany the first scientific periodical to be born was *Acta Eruditorum,* founded by Otto Mencke in 1682 and published in Leipzig, where the first German scientific society—Academia Naturae Curiosum—had started, and where printing had flourished since the end of the fifteenth century. It was not long before similar journals came into being in Italy, Switzerland and Holland. (Incidentally, many of the early scientific journals were in French, which was then the language of learning.)

It should be noted that the scientific work of those early days was a long way from being as specialized as it is today; this is obvious to anyone who

has compared an early copy of the *Philosophical Transactions* with a modern number of that journal. In those days a society like the Royal Society embraced non-scientific men, such as Samuel Pepys and John Graunt (the haberdasher who published his famous pioneer paper on vital statistics in 1662), and the scientific writings characteristic of the period were read by both scientists and non-scientists. Typical of these men was Pepys who read widely in the current scientific literature; not only did he see the *Philosophical Transactions,* but he also read many scientific books (e.g. Hooke's *Micrographia* and the various works of Boyle), and judging from his diary he evidently enjoyed reading them. Because of the nature of the scientific work done in the seventeenth century, educated men who wanted to keep pace with the progress of science could do so in the proceedings of the learned societies. A keen observer can see in the literature of the seventeenth century signs of the split into technical and non-technical scientific writing but it was to be many long years before the ultra-specialization so typical of modern science was to render communication between the different sciences so difficult and to raise equally grave problems for the 'public relations' of science as a whole.

Even in the early days there was some popular exposition, and many of the first scientists did not find it impossible to explain in simple language their ideas to the monarchs and their courts, or to their fellow-intellectuals. The first of the great popular expositors was Bernard le Bovier de Fontenelle (1657-1757). He was a nephew of the great Corneille, and we find him exercising his literary talent as a contributor to the paper which his other uncle, Thomas Corneille, edited—the *Mercure Galant.* He was a man of very wide interests, and put a great deal of hard study into every subject which interested him. On his appointment as secretary of the French Academy of Sciences in 1699, he acquired very wide contacts, enabling him to keep track of all the important new discoveries that were being made in France and abroad. He must indeed have had rare qualities, for Voltaire, the liveliest brain of the period, considered him the most universal genius he had ever met. His *Entretiens sur la Pluralité des Mondes* is well worth study even today, in spite of the fact that we have seen a long succession of brilliant popular writers of astronomical books culminating with James Jeans and Eddington. Fontenelle's is a delightful work: the whole volume is in dialogue form and the author's touch—which is perfectly attuned to the needs of the imaginary marquise, '*jeune, aimable et ignorante*', to whom Fontenelle addresses his remarks—is so sure that one can readily understand how it was that the *Entretiens* found its way into the fashionable salons of Paris, and was read as avidly as Buffon's *Histoire Naturelle* some time later. It is well said that Fontenelle '*nous enjôle à la vérité*'—for he does indeed wheedle the reader to the truth. His combination of scientific knowledge with love of literature was of course rare and unusual; few men have ever

been versatile enough to write a book like *Théorie des Tourbillons Carthésiens,* next a history of the French Academy of Sciences, and then—in a very different mood—a history of the French theatre and a biography of Corneille! His *Eloges* of great scientists also deserve to be mentioned, and it is still interesting to read Fontenelle's assessments of the importance of the work of Newton, Leibnitz, Boerhaave, Bernouilli and Tournefort. (His *Eloges* started something of a fashion; d'Alembert, for example, left behind him a collection of *Eloges* on great academicians who died between 1700 and 1722. In Britain the custom started by Fontenelle is maintained by the Royal Society with the obituaries that appear in its *Notes and Records.*)

FREEDOM OF THE PRESS—

It was during Fontenelle's lifetime that the great struggle for the freedom of the press was joined, a freedom which had to be established by custom and practice, if not actually by law, before the scientists could enjoy the liberty of publishing their findings without the fear or favour of King or Church. In 1600 Giordano Bruno had been burnt at the stake for his 'heretical' ideas, and it was not surprising that his contemporaries and immediate successors should temper courage with a great deal of discretion before they sent their scientific writings to the printers. There was, for instance, the case of Galileo, whose ideas—expressed in *Dialogue on the Ptolemaic and Copernican Systems*—were regarded by the Church as a direct challenge to her authority; and he was forced to recant 'what seemed to contradict Scripture' by the same Pope Urban VIII whose patronage he had formerly enjoyed. The progress of science required the full and unexpurgated publication of scientific results, but in the early seventeenth century this was tantamount to expecting scientists to risk their lives for their theories.

The battle for a free press was fought by the men of letters rather than by the scientists. Voltaire and Diderot were in the forefront of the struggle, and the launching of the French Encyclopedia opened the floodgates.

Up till that time a successful career as a writer required considerable discretion and luck, for any honest writer always ran the risk of offending the authority of monarch or Church and losing their all-important patronage. Any such offence, either real or purely imaginary, might in addition involve rough summary justice: in France, the erring writer could find himself imprisoned in the Bastille on the strength of a *lettre de cachet;* in England, it might mean imprisonment, the pillory or worse, after trial by the Star Chamber. To scientists who wanted to publish their results, there was a measure of 'safety in numbers' through their association in scientific societies, though their feeling of such security was more than a little shaken to see the world-famous Academia Secretorum Naturae of Naples suppressed because

of a charge that its members meddled in witchcraft, and when the even more influencial Accademia dei Lincei of Rome ran into difficulties when the Church condemned the Copernican theory.

Right in the middle of the seventeenth century very clear and democratic ideas about political power were expressed by Thomas Hobbes in *Leviathan* (1651), which thrilled intellectuals everywhere and sowed seeds of fear among all men in a position to wield absolute power. The monarchs and their ministers soon realized that the idea of constitutional checks on power would eventually undermine their authority if it ever caught the popular imagination, and the clash with the press became inevitable, for the press was the medium whereby such revolutionary proposals were being spread. It was already clear that the men of the press had acquired a measure of political power which challenged the royal and ecclesiastical authority. Voltaire and men like him took it as a basic human right that they were entitled to think about 'all things knowable and even of some others', and to publish their thoughts. Voltaire's running fight with the forces of suppression was fought with a single weapon—his pen, which he was able to wield with terrifying effect, sometimes as a rapier and sometimes as a battle-axe. The abuses of power, the pomposities and the suppressions perpetrated by the authorities were exposed to public gaze, and their eventual elimination then became certain and inevitable. Voltaire started a landslide of such magnitude that before the century was ended the religious, monarchical and aristocratic institutions were all completely transformed. Hence the famous comment that, if the seventeenth century in France was the century of Louis XIV, the eighteenth was the century of Voltaire. His fight for a free press was his greatest contribution to human liberty, a point well brought out by Lord Morley in his comment that Voltairism—the Spirit of the Age of Reason—with its freedom of thought, speech and the press would not have struck root 'if Voltaire had seen all that he saw, and yet been indolent; or if he had been as clear-sighted and as active as he was, and yet had only lived fifty years instead of eighty-four'.

—AND THE FRENCH ENCYCLOPEDIA

In the climate of opinion which Voltaire did so much to create it became possible for the great French Encyclopedia to materialize. This work was a perfect example of Voltairism in action, taking all knowledge and all philosophical ideas as coming within its scope and giving special attention to new scientific and technological developments. This was indeed a very important landmark in the history of science. Once the dead hand of the royal and ecclesiastical censors had been removed from the press, it remained to be demonstrated that progressive scientific ideas could be propagated in spite

of the reactionary attitude of the universities. The French Encyclopedia was the first big practical proof that there was a way of overcoming the stagnation of higher learning, which had developed to the point when only a few of the best scientific brains of that period could settle down in a permanent university post. Many did their researches and wrote their treatises in exile from their native country. By academic standards the most original thinkers of the period were 'rebels'; Newton at Cambridge was the exception that proved the rule.

The French Encyclopedia demonstrated that a collection of books could fulfil some of the functions of a university, with more freedom of thought and of speech than any contemporary university could boast of. The 'universal' approach of this work reflected the attitude of the great men of this period. That universality of interest was not of course a feature unique to the seventeenth century. Roger Bacon (1214-94), whose attitude to authority in scientific questions would have done credit to a twentieth-century scientist, took a thoroughly comprehensive view of knowledge. His immortal *Opus Majus* (1267), we find, was designed as the introductory sketch for an encyclopedic work—*Compendium Studii Philosophiae* (1271-72), which however was never completed. (Possibly it would have been finished had he not run foul of his superiors in the Franciscan order, who condemned him for 'suspected novelties of opinion' and kept him in strict seclusion for 20 years of his life.) Another author with the encyclopedic approach was Bartholomew the Englishman whose book *De Proprietatibus Rerum* (*c.* 1250) proved so popular that it was translated from Latin into French, Spanish and Dutch. In the early seventeenth century the *Encyclopedia Scientiarum Omnium* was published by a German named J. H. Alsted (1588-1638); this was so excellent a work that a century later Leibnitz took the trouble to work out a complete scheme for revising it and bringing it up to date. In 1727 Ephraim Chambers, an English Quaker (1680-1740), published by subscription his two-folio *Cyclopedia or a Universal Dictionary of the Arts and Sciences*. This was a wonderfully concise work resembling a modern technical dictionary rather than a modern encyclopedia, and was indeed produced as a direct competitor to the *Lexicon Technicum,* first published by Harris in 1704 and still in print in 1727. It was the arrival of a copy of Chambers' *Cyclopedia* which led Lebreton, a French publisher, to consider printing a similar work in France. This idea brought him in touch with Diderot (1713-84), who poured into the project the pent-up energy he had accumulated through years of frustration, and transformed it into a magnificent enterprise of immense scope. As one biographer[1] has put it:

> His fertile and energetic intelligence transformed the scheme. By an admirable intuition he divined the opportunity that would be given by the encyclopedic form of gathering

1. Lord Morley, *Diderot and the Encyclopaedists,* London, Macmillan, 1923 ed.

up into a whole all the new thought and modern knowledge existing as yet in unsystematic and uninterpreted fragments.

It was as though the comprehensive treatment which Agricola had achieved in his classic monograph on metallurgy *De Re Metallica* (1556) could be applied *to all knowledge*. The project as outlined by Diderot was a vast one and quite beyond the resources of a single bookseller, so Lebreton invited three other booksellers to join him and Diderot enlisted the aid of d'Alembert (1717-83) as his co-editor. D'Alembert made mathematics his special province, but he was no narrow specialist, and indeed attributed importance to science only 'because it enabled him to speak with authority in philosophy and religion'. Around these two men was created the 'encyclopedic workshop'. This workshop was remarkable in that it had the qualities of an unofficial academy with unusually strong scientific interests; the academicians included such distinguished men of science as Buffon, Daubenton, Haller and Condorcet; the great Voltaire, too, gave the Encyclopedia his complete support, always being ready to lend a hand, no matter how far below his great talents a particular topic might be. (He wrote, for example, the entry on hemstitching!)

As a publishing feat the Encyclopedia was indeed stupendous; at one stage in its preparation 50 compositors were engaged in setting the type, while a host of engravers were hard at work producing the copper plates to illustrate it. This revolutionary enterprise, which was begun in 1746 and completed in 1765, ran to 21 volumes and had a social impact far greater than any similar work has since had. It was remarkable for the coverage of scientific and technical subjects that it gave; the whole work had a rare and integrated quality, reflecting the driving force of Diderot. (D'Alembert left the project long before it was completed.) The treatment of each and every subject dealt with in the Encyclopedia was wonderfully vivid; to quote again from Lord Morley's biography of Diderot, where he speaks of the illustrative plates:

> The bustle, the dexterity, the alert force of the iron foundry, the glass furnace, the gunpowder mill, the silk calendry, are as skilfully reproduced as the more tranquil toil of the dairy-woman, the embroiderer, the confectioner, the setter of types, the compounder of drugs, the chaser of metals. . . . To turn over volume after volume is like watching a splendid panorama of all the busy life of the time. . . . The reader smiles at a complete and elaborate set of tailor's patterns. He shudders as he comes upon the knives, the probes, the bandages, the posture of the wretch about to undergo the most dangerous operation in surgery. In all the chief departments of industry there are plates good enough to serve for practical specifications and working drawings . . . [Diderot] actually took the pains to make it a complete storehouse of the arts, so perfect in detail that they could be at once reconstructed after a deluge in which everything had perished save a single copy of the Encyclopedia.

The records show that it was Diderot who insisted upon the inclusion of such technical detail. His answer to the suggestion from d'Alembert that some scholars might consider this out of place in the Encyclopedia was a categorical

statement that details of economic activity and industrial arts and crafts had as good a right to inclusion as scholastic philosophy or some system of rhetoric or the mysteries of heraldry—'yet none of these was passed over'.

Here then was the first great printed work to open the eyes of men of science to the range and the details of industrial practice, and conversely to reveal the facts and philosophies of science to the craftsman and artisan. It thus became the instrument which created not only a great intellectual but also a technical revolution in France. One final thing about the Encyclopedia needs to be emphasized: it established writing as a professional activity of the first importance, and raised the professional writers to the level of a new teaching order, an order which had more liberty of expression than had ever existed in any university or church school. Freedom to print was now recognized as a fundamental freedom, which authorities could encroach upon only by an abuse of their power so open and obvious that all men of integrity would be moved to vigorous protest.

The press was now a power in the land—the Fourth Estate, a power no less real than that of the clergy, the nobility and the *Tiers Etat*. Across the Channel a similar development took place, though the fight to establish press freedom was less fierce than in France. To be able to print at all, one had to have a licence in Britain even after the execution of Charles I, and the authorities tried to control the power of the press by keeping the number of printers and master type-founders down to a bare minimum. The licensing authority was a curious body—headed by the Archbishop of Canterbury and the Bishop of London, and the right 'to search for scandalous pamphlets and to seize lying pamphlets, presses or printers' was only curtailed by Parliament after Milton published his famous declaration of liberty, *Areopagitica* (1644). Even though the licensing of presses was strictly enforced in England until 1694, scientific publication did not suffer from the same restrictions that applied in France. By the beginning of the eighteenth century the English press was ready to contribute its share to the Age of Reason, and science was to benefit accordingly.

SCIENTIFIC JOURNALS IN THE AGE OF REASON

Some students of the history of science are inclined to give most of the credit for the great burst of scientific activity in the period 1660-1760 to a spreading of the understanding of 'scientific method'. Yet the records show that qualitative method was fully appreciated in the Middle Ages; it was quite clearly defined by Robert Grosseteste (*c.* 1175-1253), and was certainly understood by his student Roger Bacon. There are some who argue that such early scientists were in effect unscientific in that they used the experimental method not in a search for truth but because they were concerned with the

exposition of scriptural knowledge. But as late as the nineteenth century some men who were indisputably scientists were inspired by the same motive. Random reading of seventeenth-century literature can give a most misleading impression as to the importance of scientific method in that period when scientific research on modern lines was gaining prominence; and the contemporary writings by non-scientists about scientific things may lead one to dangerously wrong conclusions. The purely theoretical publicists of scientific method were able to devote more time to writing than the practising experimentalists; they left a superabundance of words behind them and one is tempted today to read into their writings a great deal more than did contemporary experimentalists, especially since we are always liable to supplement the words of such writers with the wisdom of hindsight and our twentieth-century knowledge of scientific method. Any piece of scientific writing may have an impact when it is first published which is vastly different from the impact it has at a later period. To take a fairly modern example: Darwin and Wallace presented their paper on the origin of species by natural selection to the Linnean Society in July 1858, and it caused hardly a ripple in the world of biology: in November 1859 Darwin's *The Origin of Species* was published, and it caused an intellectual revolution. The theory advanced in 1858 was essentially the same as that of 1859, and yet the impact was utterly different. This illustrates the allowances historians of science must make when they are trying to assess the contemporary impact of some early scientific paper or book. Unfortunately, only a few students of the history of science are sufficiently critical in their appraisial of such literature. One risks being misled by taking printed historical material and accepting the statements and opinions at their face value, without due regard to the historical context.

It seems reasonable to suggest that it was not from the printed word of the seventeenth century that the essential qualities of the experimental method were learnt. The full quantitative experimental method was implicit in the work of Galileo, whose approach to scientific problems had much in common with that of the modern mathematical physicist, and Galileo was apparently inspired to add measurement to qualitative experimental method by the precept of Archimedes, one of the first men to search for mathematical laws in physical phenomena. In the seventeenth century the understanding of experimental method was almost certainly spread by word of mouth and by practical demonstration rather than by printed accounts. Surely if the English king learnt about William Harvey's discovery of the circulation of the blood by watching Harvey's demonstration and listening to his verbal explanation, the scientific students of that period must have acquired most of their appreciation of the value of scientific method direct from its practical masters. One cannot help feeling that much of the current arguing about the great contributions of Francis Bacon to the development of modern science

—in particular as an expositor of the 'new experimental philosophy'—is fruitless, because it is based on inadequate knowledge of the factors which controlled scientific progress at that time. One would do well to ponder the remark which Harvey made about Bacon: 'The Lord Chancellor writes about science—like a Lord Chancellor.'

This issue has been gone into at some length because it seems important to try to assess the real value of the printed word to the scientists of 1660 onwards. In the main, it seems fair to conclude that the essentials of scientific method and the detailed 'know-how' of experimental research techniques were both taught most successfully by the same sort of method which enables the beginner to learn them today—by practical work. Scientific periodicals rapidly became the main method of communicating the results of research, and were soon indispensable 'tools of trade' to the scientific worker. Discoveries were being made at an ever-accelerating rate, and scientific periodicals were thus born of sheer necessity since books could not be written or printed quickly enough to benefit the original investigator. The early scientific journals acquired their modern international character in a surprisingly short time: the *Philosophical Transactions,* for example, soon after its foundation in 1665, started publishing items of foreign scientific news, some picked up by scientists on their travels, others submitted by foreign scientists.

In those stirring days when science was getting out of low gear, the scientific periodicals rendered great service in another way: they helped to open the individual scientist's eyes to many new facts and phenomena which would have remained beyond his ken had he been working under the conditions of isolation which obtained at the time of, say, Roger Bacon. This undoubtedly had a stimulating effect on observation and experimentation, thereby increasing the range of phenomena that came under scientific examination. The seventeenth-century scientists were thus able to add to the collective pool of published knowledge, and this in turn led to an increase in the rate at which fruitful hypotheses could be formulated and tested experimentally.

Many members of the earliest scientific societies were not practising scientists, and the journals published by those societies thus served another useful function by maintaining contact between the scientists and the many influential laymen who were sufficiently interested to join the societies. It is even possible that as a result of this the linkage of the governing class to the scientific world was better in the time of Samuel Pepys than it is today: it is certainly true to say that Pepys, because of his close contacts with scientific men who were fellow members of the Royal Society, was exceptionally successful as an administrator in the British Admiralty, being able to initiate some scientific investigations into specifically naval problems which otherwise would not have been recognized as capable of scientific investigation.

With the wider dissemination of scientific knowledge, many industries stood to gain, and here special societies and new journals played important roles. One of the first of these societies in Britain was the Royal Society of Arts,[1] founded in 1754. A less formal but perhaps even more influential group (at least at the time of its original formation) was the famous Lunar Society, which brought together industrialists and others with scientific interests in the Birmingham region. The members included Dr. Erasmus Darwin, Joseph Priestley, Samuel Galton, James Keir (who translated Macquer's *Dictionary of Chemistry*), Dr. William Withering (who did pioneer work on digitalis and other drugs), John Whitehurst (the horologer), John Baskerville (the printer), Matthew Boulton and his engineer James Watt. Most of the members of the Lunar Society were industrialists, and the group was in fact rather less catholic in its interests than the unofficial 'academy' which compiled the French Encyclopedia. But its meetings served to forge a valuable link between science and industry, and it is fair to claim that the Lunar Society was productive of 'experiments, inventions and discoveries that have changed the face of civilization'.

The Lunar Society and similar bodies did much to make up for the neglect of science in British universities. In Manchester, a similar organization which brought scientists and industrialists into personal contact was the Manchester Literary and Philosophical Society, the influence of which spread far and wide when the society started its journal in 1785. This served as the prototype for other provincial societies 'which were formed at the rate of one, five, and even ten per decade in the Victorian period'. In London it was the Royal Institution in particular which forged links between industry and science.[2] The scientists on the staff of the Royal Institution were able to concentrate on fundamental research; but diffusion of scientific knowledge has always been abundantly carried out, and the institution's *Proceedings* help to spread the words of the R.I. lecturers far beyond the lecture room. A magnificent tradition of exposition, both oral and printed, is associated with the Royal Institution. This began in the days when Davy lectured there; he was a brilliant expositor who attracted all fashionable London to the Royal Institution, in much the same way as Rouelle, the eccentric French chemist who inspired both Lavoisier and Proust to take up chemistry, used to fascinate the cream of Paris society with his chemistry lectures at the Jardin du Roi. Many of the Royal Institution lectures were made up into book form, which extended their influence considerably. Some of the finest popular science books ever published in Britain originated in that way: Faraday started the famous series of children's lectures which are given each year

1. The full title is the Royal Society for the Encouragement of Arts, Manufactures and Commerce. A bicentenary volume entitled *The Royal Society of Arts: 1754-1954*, written by Derek Hudson and Kenneth W. Luckhurst, has just been published (London, John Murray).
2. Later, such bodies as the Chemical Society were to play an important role in this connexion: later still came the professional institutions for engineers and other technologists.

during the Christmas school holidays, and one of these courses of lectures was published by Faraday as *The Chemical History of a Candle,* which became a popular classic. His successor John Tyndall produced in this way an equally fine book—*Forms of Water* (1874). A high proportion of the R.I. children's lectures have been printed since Faraday started them, and it is fair to say that the only reason why some of these lectures remain unpublished in book form is the failure of the lecturers concerned to provide a script. More than one publishing firm is ready to print each and every set of lectures as soon as it becomes available.

THE PROBLEM OF SPECIALIZATION

By the beginning of the nineteenth century daily newspapers were well established, and in addition there were a number of vigorous periodicals. Journalists with a good sense of what interested the public started to record the happenings in the world of science. The coverage of this kind of news grew even more extensive when societies such as the Deutscher Naturforscher Versammlung (which first met in 1822, in Leipzig) and the British Association for the Advancement of Science were started. The meetings of these organizations began to be recognized by newspapermen as of first-class interest to the press, and their proceedings have always been reported at great length by daily newspapers—even by those which seem to ignore science altogether throughout the rest of the year. The British Association was particularly shrewd, and took advantage of the interest of the newspapers in its meeting; intentionally, it chose to hold its annual conference in the late summer, when it would derive the fullest possible benefit from the general dearth of hard news at that time of year.

The various associations for the advancement of science have all aimed at the strengthening of the ties between the world of science and the general public, and they have recognized that for this purpose the help of the press is indispensable. This still remains true, even since the coming of radio and television.

Another aim of these associations—which embrace all the sciences, including economics and psychology—is the provision of a common meeting ground for all scientific specialists. Some of the bad effects of the ever-increasing specialization of science are thus neutralized. The fragmentation of science, and the formation of specialist societies with journals covering only a very restricted field, have led to a situation where botanists and zoologists find themselves strangers, chemists tend to be shut out of the world of physics, and economists, psychologists and sociologists are liable to be neglected altogether by the majority of professional scientists, even though their sciences could do much more than they do at present to correct some

of the more unfortunate consequences of scientific and technological advances. The paradoxical result is that a scientist working in one field may be quite unable to follow a paper written by an old school or university friend who has specialized in some other field.

Increased specialization has been accompanied by a tremendous rise in the cost of scientific research, until it has reached a point where only governments and rich foundations (such as the Nuffield, Carnegie and Rockefeller foundations) can foot the bill. This large expenditure on science needs to be justified before the bar of public opinion, and here such bodies as the British Association in their capacity of 'parliaments of science', meeting in open sessions which are attended by newspapermen, can do a great deal of good. There have certainly been occasions when the British Association succeeded in stirring public opinion on fundamental issues. For example, in its early days it was able, with the aid of the press, to arouse the public to the realization that a continued neglect of science would reduce Britain's industrial power and undermine her position as a world power. Within 20 years the lesson was being turned to account, and by the time of the 1851 exhibition in London most aspects of the scientific and technical scene were far more encouraging than they had been in 1830, when Charles Babbage produced his challenging pamphlet *Reflexions on the Decline of Science in England*. In the same way the British Association (BA) argued the need for a National Physical Laboratory (NPL); this suggestion was first made by Sir Oliver Lodge in an address to the BA in 1891 and was widely reported; the upshot was a government committee to investigate the proposal and then, in 1900, the setting up of the NPL at Teddington.[1] More or less throughout the whole of its career, the British Association has enjoyed good relations with the press, and there is little doubt that much of its success in influencing official policy towards science is due to that fact.

The breadth of interest of the British Association was in sharp contrast to the ever-growing specialization. Everywhere new specialist societies were springing up rapidly in the nineteenth century, and most of them brought out highly specialized journals. To cite but one example, the Chemical Society which was born in London in 1841 started publishing *Memoirs* as soon as it was founded, and later produced a *Journal* and *Proceedings*. Finally, it met its members' demand for periodical *Abstracts* which would simplify the reading of new literature, guiding the individual searcher to those articles in all the world's chemical journals which his specialized professional interests required him to read. Another very large and wealthy society—the American Chemical Society—could afford to produce a big range of journals, including

1. Other instances are given in O. J. R. Howarth's *The British Association—A Retrospect: 1831-1931*; in particular in the chapter 'The Association and the State', p. 212. The teaching of science in schools was early advocated by the BA (1864-67); the BA also fought for increased expenditure on university education, particularly scientific studies (1903-04, when the government accepted its ideas which were backed as usual by informed public opinion).

The Journal of the American Chemical Society (1897), *The Journal of Chemical Education, Industrial and Engineering Chemistry, Analytical Chemistry,* and *Chemical Abstracts* (a fortnightly series of abstracts started in 1907). Chemical specialisms required their own journals: an early example was the *Zeitschrift für Physikalische Chemie,* which was started by Ostwald in 1887 and without which some of the early classic papers on physical chemistry might never have found their way into print, 'because chemists would not recognize them as chemistry, nor physicists as physics'. The building of the bridge between chemistry and physics has proved most important to the development of chemistry, both pure and applied, and it has also revolutionized the approach to certain chemical problems that arise in biology; one might well say that the scaffolding upon which that bridge was erected was provided by Ostwald when he started this new journal. New specialisms in the chemical field led to new chemical industries, and this in turn stimulated the commercial publication of more and more journals catering for the chemical industry.

Some idea of the vast range of scientific journals now published can be gained from the *World List of Scientific Periodicals,* the latest edition of which weighs just on eight pounds and contains some 40,000 titles. In 1934 it contained 33,000 titles. The latest *World List,*[1] covering scientific journals published between the years 1900 and 1950, contains 10,000 more titles than the first *World List* published in 1925.

Keeping up with the literature has become a major problem, so that a new type of professional specialist—the scientific librarian and the scientific information officer—has come on the scene, and there are now even one or two specialized journals to deal with his special problems. In addition, quite a large number of scientists devote some of their time to producing abstracts of current literature, without which the modern scientist would find the search for even the new research papers of immediate professional interest to him almost as hopeless as looking for a needle in a haystack. In certain branches of science, many research men hardly ever read a journal at all, but live on a diet of 'separates' (reprints) and the few papers which they track down through their abstract journals. But this is by no means true of the great majority, and to most members of scientific societies the society's journal is indispensable: so much so in fact that one frequently meets scientists who have joined a society simply to obtain its journal, without any intention of attending any of its scientific meetings or social functions. There are a few societies which hold no meetings and simply exist in order to publish: for example, the Ray Society has no other aim than bringing out an annual monograph; it is thus only a means of getting regular subscribers for expensive monographs which commercial publishers would find difficult to market.

1. *World List of Scientific Periodicals,* edited by W. A. Smith, F. L. Kent, and G. B. Stratton, Butterworths Scientific Publications, London, 1952.

The cost of publishing has become a considerable item in the balance sheets of most scientific societies. Taking at random two societies in London, one scientific and one technological, i.e. the Linnean Society and the Institution of Chemical Engineers, the figures work out in both cases at approximately 30 per cent of their annual incomes. There is every reason to think that this figure is typical of most similar societies in Britain.

THE DANGER OF RATIONALIZATION—

Many scientists grumble at the amount of money they have to spend on books and periodicals, especially as the income tax authorities often do not recognize all such expenditure as tax-free allowance for indispensable 'tools of trade'. For this reason, and because of the great amount of time which they have to spend in searching the literature, the superabundance of scientific literature is something which the scientists are inclined to regard with a jaundiced view. Everyone agrees that it does represent a tremendous problem nowadays, when it may prove quicker to re-discover a known fact than to search and find that fact in the published literature. A typical opinion on this issue is that of Professor J. D. Bernal:[1]

> In spite of all attempts at rationalization there is still an enormous amount of overlapping and gaps in abstracting work, and abstracts themselves have reached an unwieldy size. Thus *American Chemical Abstracts* consist every year of three volumes of 2,000 pages each, with an index in addition of 1,000 pages. This situation is growing rapidly worse. . . . The result is that it has become impossible for the average scientific worker, who does not wish to devote the major part of his time to reading, to keep up with progress in his own field, and almost impossible for anyone to follow the progress of science as a whole even in the most general way. At the same time a large quantity of good scientific work may be permanently lost because it was not appreciated when it was published, and subsequently everybody has been so busy in keeping up with recent publications that there has been no time to sift through the records of the past.

Some scientists, among them Professor Bernal, are demanding a drastic reform of scientific publishing. Thus in the very first issue of *The Journal of Documentation* (first published in 1945), a periodical devoted to the 'recording, organizing and dissemination of specialized knowledge', N. W. Pirie suggested the following 'rationalized' scheme: there should be some financial arrangement whereby a hundred or so British journals could be interlocked to the extent that one subscription would entitle the individual subscriber to receive on request a reprint of any paper from any of the other participating journals. This, in effect, would mean that scientists would subsist solely on a diet of 'separates', at least as far as British literature is concerned. The same need prompted the Royal Society Empire Scientific Conference of 1946

1. J. D. Bernal, *The Social Function of Science*, London, 1939.

to devote one whole session to a discussion of measures for improving scientific communications within the British Empire, and to delve into such technical issues as indexing, abstracting, the organization of specialized libraries, and microfilm.[1] Micro-cards have since become as important as microfilm, and there are many who believe that they provide an ideal way of reducing the physical bulk of printed scientific literature, so that far less space need be allocated by laboratories to library accommodation.

'Rationalization' of publication—which would almost certainly involve some regulation of publication—seems desirable on the grounds that it would make for greater convenience of reference to the literature. But the apparent advantage might well be offset by other things, just as the apparently rational system of standard abbreviations used in giving the titles of scientific journals in the lists of references published at the end of research papers is in fact more nuisance than it is worth because of the enormous and unnecessary trouble which it causes editors, compositors, correctors of the press, and the men who actually make up the pages of scientific journals on the stone.[2] If the 'rationalists' go as far as to advocate a reduction in the number of scientific journals that are printed, there would arise one great danger which was practically eliminated when the modern multiplicity of journals came into being. In the days of only a few scientific journals, quite a number of first-class research scientists found great difficulties in getting their work published. There was the classic case of Joule (1818-89), whose main papers in his early and most productive years failed to achieve publication by either the Royal Society or the British Association, to which he had read the papers in question. This neglect might have been most serious had it not been for the fact that the *Philosophical Magazine*,[3] founded in 1798 and still edited and published privately, came to his rescue. A similar case was that of F. W. Lanchester, whose revolutionary aerofoil theory of about 1894 went unpublished for a long time after he had explained it in a lecture to the Birmingham Natural History and Philosophical Society. A paper which he wrote on it in 1897 was rejected by the Physical Society, and his ideas did not get a real chance to make a significant impact until after they appeared in book form—Lanchester's classic *Aerial Flight*—in 1907. Scientific and technical progress can in this way be seriously hindered, and even a published paper may become 'lost' for all practical purposes. Thus Avagadro's hypothesis, first published in 1811, was not recognized at its true value until 1859 when Cannizzarro rescued it from obscurity. The same thing happened to

1. The proceedings of this particular session can be consulted in *The Royal Society Empire Scientific Conference*, vol. I, published by the Royal Society, 1948.
2. There is in addition the time the author has to waste in looking up a list of standard abbreviations; decoding the abbreviations can also waste time, and specialist librarians seem to be the only people who have a good word to say for these symbols.
3. This fine independent journal, with its impressive record of great receptiveness towards new and unorthodox work, came into existence by the coalescence of *Nicholson's Journal* and the *Annals of Philosophy*.

Mendel's classic papers. Mendel's Laws had in fact to be completely rediscovered before the importance of Mendelism could gain general recognition; had Mendel's original papers been appreciated at the time of publication, they would have gone a long way to exploding the ideas about blending inheritance that were compounded with Darwin's evolutionary theory to produce a morass of mistaken theorizing in which biology remained bogged down for so many years.

It is a safe working axiom that no editor is perfect, for the judgments involved in editorial work are matters of extreme delicacy, and no one can be assumed to have an infallible instinct automatically leading him to take the right decision. Even when the editor's responsibility is fragmented and shared out among the members of an editorial board or a panel of referees, delicate decisions still have to be taken by equally fallible human beings. Possibly the problem might be solved where second-rate papers were being considered, but personally I doubt whether either editors or editorial boards ever have much difficulty in recognizing the papers that need to be rejected. It is the papers that are difficult to understand which present the tricky editorial problems, and the difficulty of following an author's line of thought may be solely due to the fact that he has hit upon an original and unfamiliar idea. There is always a distinct risk that the most conscientious editor or editorial board will fail to recognize the true value of a paper in the Joule-Mendel class, and for that reason it is an advantage to the truly original scientific thinkers that there should be a rich variety of journals to which they can submit their work; the more journals there are, the less the likelihood of a particular paper languishing in a scientist's bottom drawer or in the archives of a learned society.

—AND THE NEED FOR CONCISENESS

The rich variety of journals may be an embarrassment to searchers of the literature, but it benefits the real researchers who might otherwise encounter difficulty in bringing their discoveries to the notice of their fellow workers. Possibly some of the energy which is spent worrying about the multiplicity of journals, and ways of eliminating some of them, could be more usefully applied to the preparation of better research reports and to the better editing of existing journals. Many scientific documents are far longer than they need be, and their lack of succinctness reduces their impact. This point was stressed by L. J. F. Brimble, joint editor of *Nature*, in his address on 'Science and the Press' to the Royal Society of Edinburgh (11 January 1954):

> It was the late Lord Rutherford who once said that, when writing a letter to *Nature*, if you cannot say all that is really necessary in 500 words or less, then something is

wrong. If every scientist throughout the world believed this and took it to heart, then I can visualize even *Nature* having no time lag in publication at all. It is significant that though *Nature* frequently returns a communication to an author with a request that it should be reduced to two-thirds or one-half its length, I can only recall about two instances in the whole of my twenty-five years' connection with *Nature* of an author replying that he could not cut his communication.

My own personal experience on *Discovery* confirms the general truth and justice of Mr. Brimble's remarks, and I agree with his suggestion that scientists ought to ask themselves three questions before submitting anything for publication:

1. Are you sure you have said what you want to say?
2. Have you said it in the minimum number of words?
3. Is it worth saying at all?

Prolix writing is in fact a social sin when it is perpetrated by scientists, for it simply blocks the channels of communication; in this way the prolixity of the worst papers has the effect of delaying the publication of the best, for which reason the finest scientific brains find their relations with editors rather sweeter than do the coarser.

The best scientists have nearly all been masters of the art of concise writing; Rutherford was a fine example. He had very much the same views on conciseness and prolixity as most editors, through bitter experience, come to hold; he once justifiably castigated an author for writing a book that was four times longer than it should have been, and he particularly stressed the fact that the book would have been that much more effective had it been four times shorter.

It does seem that higher editorial standards all round are called for, and the responsibility for improving the quality of scientific journals generally rests with both the editors and their contributors. Some scientific societies go to great trouble to brief their authors; I believe that the detailed 'instructions to authors' which a great many scientific journals now send to their contributors is a step in the right direction, and it is to be hoped that the practice will be extended. There is one very material reason why everything possible should be done to improve the standards of factual presentation in scientific journals; higher editorial standards make for smaller printing bills, and that is a vital consideration in these days when scientific societies find that something like 30 per cent of their income goes on printing their literature. The same thing applies to scientific books, for here the high retail price is having a very adverse effect on the number sold; financially the author may not suffer, but in terms of prestige and influence he loses if his ideas in book form can only reach the people who can afford to pay high prices.

In the scientific world it is being increasingly realized that the literary men were right when long years ago they pointed out that clear writing is an expression of good manners, and that ability to write well is a distinct social

and professional asset. Typical of recent books written to bring this point home to students of technical subjects is Professor R. O. Kapp's excellent work, *The Presentation of Technical Information*. Another British effort in the same direction is associated with Edinburgh University, where Professor James Kendall takes pains to see that his undergraduate chemists develop an effective literary style; he finds it useful to get them to produce one essay per term on a theme sufficiently general in character to give them room to marshal their ideas clearly. (The importance of general essays as opposed to essays on technical topics cannot be overstressed, for it is the first type of essay that can best restore the ability to write well, an ability which often withers in undergraduate years when students begin to acquire a 'laboratory notebook style', spending a great part of their remaining time cramming a never-ending succession of verbless sentences down in their lecture notebooks.) The attention that is now being paid to the improvement of the presentation of scientific information and the practice which scientific undergraduates in numerous universities all over the world are getting, are both bound to pay big dividends in the future; we can look forward to a time when the standard of literary style will be higher than it is today, and almost certainly the undergraduates who are now being encouraged to write well will raise standards still further when their generation assumes the editorial control of the journals of the scientific societies. At institutions such as the Massachusetts Institute of Technology and the California Institute of Technology in the U.S.A. the technological students study literature and literary style; moreover the students' essays on purely technical subjects are all liable to criticism for any literary lapses, quite apart from factual errors and faults in logic.

Another suggestion to ease the strain on publications which Mr. Brimble put forward in his Edinburgh address is this: scientists should publish only the gist of a new idea and file the greater part of the associated detail. To a limited extent this idea is already in operation, but if such an arrangement is to be generally extended (the filing can be done in the archives of scientific societies and other institutions) the decision as to what should be published and what should go into archives should be left to the authors. These decisions are essentially of a kind that does not fall within the province of editorship; editors are accustomed to ask for the removal of irrelevancies, but what arises here is a problem of censorship, and one is expecting the impossible if editors are to combine the creative function of editing with the negative function of censoring. Personally, I could never bring myself to tell a scientist that part of his paper should be filed in the archives of some society, though I would without any compunction tell an author that his article was no good at all. To me, files and archives are virtually dead papers which only the genius of the historian can bring to life. No filing system has ever been devised that is capable of taking two utterly unrelated sets of facts

and synthesizing a brand new idea out of them. Certainly machines can be made to do many things, but it is doubtful whether any machine will ever be produced that is capable of the flash of the original thought, the 'brain-wave', which human beings can experience. Published facts will be read by human beings, and who knows what unexpected 'brainwaves' may then result; but only on very rare occasions are archives consulted (indeed in several scientific societies the items in the archives are not even properly catalogued because they are never consulted) and hence there is but very little prospect that the factual information they contain will ever be synthesized into fruitful theories and hypotheses. That kind of prospect is enough to make Joule and Lanchester turn in their graves, and the spirit of Mendel might well be provoked to murmuring 'Nobody appreciated my ideas even though they were printed and circulated to a great many scientists. What chance would my work have had if it had been put in the archives instead of being printed!'

Publication in an important scientific journal has a prestige value, in particular to the younger scientist who wants to carve out an effective career for himself. In the very competitive and highly professionalized world of modern science, published papers are steppingstones to success. For this reason some scientists are excessively eager to get their work into print. This point was stressed by Mr. Brimble in his Edinburgh address:

> There is a tendency these days among scientists to rush into print. More often now than ever before some scientists, having submitted a communication for publication, eventually ask to withdraw it or to be allowed to modify it because they have either discovered an error or have since learnt that some of the work has been done elsewhere. This tendency is also revealed in the appalling state of corrected proofs received from some authors—sometimes peppered with corrections and changes which are anyhow very expensive to make. Rushing into print is also inspired by the bugbear of priority. For example, the request by an author that his communication should be treated as urgent, because he has learnt that similar work is being done (usually in the United States) is now treated like the cry of 'Wolf! Wolf!' It is happening far too often. How refreshing it is when one team of workers, having heard that another is working along the same lines, gets into touch with the second group and arranges a joint communication!
>
> There is much complaint today concerning the amount of scientific literature which each scientist must read if he is to keep pace with his own subject. There are many reasons for this overwhelming spate, not the least of which is the over-enthusiasm of scientists themselves.

The danger even arises that the amount of printed matter a scientist produces is taken as an index of his scientific ability, or—what is worse—of his scientific productivity, which must ever remain a purely qualitative concept. To administrators remote from the research laboratory the sheer bulk of printed papers a scientist produces may seem impressive. The revulsion of the true scientist to the uninformed 'bureaucracy' of science which exists today is well expressed in this passage from Leopold Infeld's book *Quest*,

which is revealing because it is an unusually frank autobiography for a scientist:

> [The pressure to publish] is much stronger in America than Europe. It may be illustrated by this authentic fragment of an interview between a department head and a famous scholar looking for a job.
>
> The head of department asks:
>
> 'What is your average production?'
>
> The candidate looks puzzled. He does not understand the question.
>
> 'It is very simple. How many papers have you published?'
>
> 'I never counted them. But I believe somewhere around twenty-four.'
>
> 'And how long have you been in the teaching profession?'
>
> 'Seven years.'
>
> 'Now let us see. Twenty-four divided by seven is 3.4. We can say that your rate of production is between three and four papers per year.'
>
> 'I see.'
>
> 'Now are you sure that you will be able to keep up this rate of production in the future?'
>
> I might add that the candidate obtained the appointment although he refused to guarantee the rate of his intellectual production.

What would such a bureaucrat have thought on counting the pages in the very succinct papers written by the Einsteins of this world!

To many a research scientist, the writing up of his results is something bordering upon drudgery, and such men do not waste words. Typical in this respect was Charles Darwin, who used to find writing an agonizing effort, though one has to read his autobiography to discover this fact, for it is the reverse of obvious from a study of his published works. In his autobiography he repeatedly talks of the difficulty of expressing himself clearly, and never did he achieve fluency when writing—though his polishing and re-polishing of sentences prevents the reader of his books detecting that lack of fluency. A just assessment of Darwin's style may usefully be quoted here—it is given incidentally by a journalist whose interests are essentially literary and not scientific:

> Darwin would have disclaimed any right to be considered as a literary artist, yet the clarity of his style, and the very quietness with which he presents his conclusions give to much of his writing the quality of a work of art.[1]

Darwin's style seems to be worthy of special study because *The Origin of Species* has reached a bigger public than almost any other scientific book. Darwin made no concessions to the cause of popular appeal beyond lucidity of thought and lucidity of expression. The arguments he advances are concentrated in the extreme; as Sir Arthur Keith has remarked, 'so much is packed into every sentence and every paragraph that the reader's mind becomes satiated unless he proceeds slowly and keeps his understanding busy'. *The Origin of Species* was never designed for the popular market;

1. A. J. Lawrence, 'Science, Literature and Language', *Discovery*, June 1949.

Darwin wrote it as a scientific treatise with the sole intention of putting his case to his fellow scientists. Yet it reached a far bigger public than the author or his publisher ever intended. The secret, I believe, of Darwin's success with this one book was his plain, unpretentious but most lucid style. In the printed word he achieved something very near perfection, and *The Concise Cambridge History of English Literature* comments that Darwin 'in his simple and almost religious way . . . said a first word of such power that the year 1859 still marks an epoch in the history of thought'. Darwin is thus appreciated by the world of literature and the world of science, which is rare indeed. While there is truth in the general remark[1] that 'it seems that we must reconcile ourselves to the inescapable observation that a study of modern science is destructive of the ability to write good English', there is no reason whatsoever why science students should not turn back to the pages written by some of the great masters of 'scientific' English, such as Darwin, Faraday, Tyndall, T. H. Huxley and John Wilkins, the Warden of Wadham College, Oxford, who played so active a part in the early days of the Royal Society and who led the movement to develop a new form of writing suited to the needs of scientists.[2] (Wilkins's sentences were short, pointed and exact).

The ornate classical style which the ruling class and the literary men of the seventeenth century affected gave way before the advance of a plainer kind of prose, which John Wilkins and his fellow experimental philosophers did so much to develop. This is the kind of prose which the great literary figures of the next century—notably Defoe and Swift—wrote to the greater glory of English letters. A similar development occurred in other countries. For example, a tradition of good writing was established among French scientists, starting with Fontenelle and extending down the years through the writings of such men as Pasteur and the present Prince Louis de Broglie. In the early days of modern science French was the dominant language, and Germans such as Leibnitz wrote everything of importance either in Latin or French; the Berlin Academy conducted its transactions first in Latin, and then for many years in French. One of its early presidents, Jablonski, thought German a noble but horribly barbarous tongue, unsuited to the needs of intellectuals. Germany lacked for a long time men of science and men of letters who could bring to the art of writing 'the vivid and radiant force of either Voltaire or Diderot', and it took all the brilliant precepts of writers

1. Made by a book reviewer in *Discovery*, April 1951.
2. John Wilkins (1614-72) was associated with the 'Invisible College', the association which met at Gresham College, London, to discuss the then 'new philosophy'—as experimental science was called. In November 1660 he was present at a meeting of this group of philosophers when the suggestion was discussed that they should form 'a Colledge for the promoting of Physico-Mathematicall Experimentall Learning'. This 'Invisible College' or 'Philosophers' Society' was the precursor of the Royal Society. At that historic meeting 'Dr. Wilkins was appointed to the chaire'. Dr. Wilkins's writings included *A Discourse concerning a New Planet* (1640) which was an exposition and a defence of the Copernican theory.

like Lessing, Wolf and Goethe before a German prose style comparable to the British and the French was evolved.

SCIENCE FOR THE NON-SCIENTISTS

So far the discussion has been confined to the scientists' use of the printed word for communicating the results of their research to fellow workers. But today science affects the lives of everyone to such an extent that the general public is entitled to intelligible progress reports showing what science is trying to do and what advances it has made towards its objectives. Since science has achieved the revolutionizing power it now possesses, it is no longer desirable for scientists to operate within 'invisible colleges', as did their predecessors of the seventeenth century who would have been risking persecution had their researches been conducted in the full blaze of modern publicity; nowadays the 'ivory tower' scientists look too much like the 'open conspirators' of H. G. Wells, for any man who produces a discovery or invention capable of having far-reaching social consequences cannot escape his responsibilities. The least of these is the responsibility to place his work before the bar of public opinion. That is what men like Giordano Bruno did in the sixteenth century, and they did so at the risk of their lives. Today the scientists enjoys the right to carry out research, and in many cases he is supported financially by the very community which stands to suffer real inconvenience and even social disturbance if the results of his work prove to have far-reaching consequences in industry, in politics or in the field of ideas. The public today are shareholders in the business of scientific results, and they are entitled to receive the kind of information which good firms disclose to their shareholders. In the words of Sir Ben Lockspeiser, the present secretary of Britain's Department of Scientific and Industrial Research, which today spends some £5 million of public money each year, 'science needs a well-informed public. It must have public opinion on its side, for unless it is nurtured in a friendly and encouraging atmosphere it will never be able to satisfy people's needs and desires in the way it should. Reliable and readable writing on scientific matters is essential'. This view is accepted as an axiom by most scientists of note, though one does just occasionally come across exceptions.

Reasonable privacy is, of course, essential for anyone engaged in scientific research, but that does not absolve the scientist from his duty to render public account of the money the public gives him. The public is entitled to a reasonable amount of information about all researches supported out of public funds. The research worker who finances his own work may possibly be entitled to take a different line, but even here a reasonable degree of publicity is desirable if only to kill the idea that he is engaged in some dangerous

and secret work: in an age of atomic bombs, the scientist who insists on complete secrecy without reasons which can be readily understood by the public always runs the risk of being regarded as a sorcerer's apprentice whose discoveries may get out of control to the peril of the community. Publication for the non-scientist has thus become an issue of great importance: information about the progress of science should be put before the public as a matter of right; it can no longer be regarded as a matter of privilege to be granted as, when and if the scientist so decides. The scientist with a social conscience realizes all this perfectly well, and it is only fair to say that such men have always acted accordingly; the generosity of some of them in this connexion cannot be over-praised, and one is humbled whenever a great scientist takes infinite trouble and spends a great deal of time to explain to one or two journalists exactly what he is doing. There are many of us in the profession of science writing who regard such praiseworthy disclosure, often of published work, as though it were a matter of courtesy rather than of public right: for the happiness of all concerned it needs of course to be both.

The main medium whereby reports of current scientific progress reach the public is, of course, the press, using the word here in its modern restricted sense of daily newspapers and mass-circulation magazines. And here it should be said that scientists need to be public-spirited and accept the 'arrows of outrageous fortune' which befall some scientific stories when they are handled by some inferior newspapers. No journalist worth his salt would condone the bad treatment which scientists have received and still receive at the hands of certain sensational journals; most journalists deplore, for example, the bad habit of concentrating on the sensational aspects of stories which are interesting enough in their scientific novelty. These points must be conceded to scientists, for they are part of the record which we journalists realize we have to live down.

The public relations of science so far as the press is concerned are like a two-cylinder engine of popularization: on the one hand, we have the scientists; on the other we have the science writers, and also those general reporters who are sometimes sent out after a scientific story. It is all too clear to the scientists that in this connexion journalists are not perfect, but it is equally clear to the journalists that the scientists are not perfect either. But if it can be accepted at the outset that both are doing their best in spite of the blinkers that restrict the vision of all concerned, there is sufficient common ground to enable both parties to co-operate and improve the public relations of science. (This principle also applies to radio producers, film directors and television men who handle science programmes, with whom journalists have so much in common that many of them give up writing to work in the newer media which have such great potentialities.)

Thoughts about popular science are today in a state of complete confusion

so that certain scientists, with some justification, consider it a matter which does not bear thinking about at all. A hint of the extent of this confusion is conveyed by the fact that we frequently see the word 'popular science' used in cases where it clearly does not apply. For example, a great many of the so-called 'popular science' books published in Britain sell no more than a miserable 1,500-2,000 copies, which means that their popularity is far below that of Darwin's lesser-known work, *The Formation of Vegetable Mould through the Action of Worms,* of which 8,500 copies were sold in about two years. Popular science is a thoroughly unhappy term: scientists are inclined to use it as a term of contempt (with good cause, I feel), while publishers put it in their blurbs as an expression of a hope that is all too often disappointed—which prompts one to wonder when an honest publisher will emerge to coin the term 'a book of unpopular science'. His honesty might be rewarded by bigger sales—who knows? To the potential reader the term is often downright misleading to the point of fraud. It must however be admitted that no alternative term has yet been found to convey the sense originally implied by 'popular science'. One finds some people using such terms as 'non-technical', but this is no more satisfactory in the case of writings that are solid technical jargon from beginning to end. When one starts thinking, one is forced to recognize that 'popular science' is nothing more than a technical term—and a bad one, in that it is misleading and was in fact obsolete almost before it was coined.

THE INTEREST OF THE PUBLIC

All science writers must start on the following basis: decide on the readership one is trying to reach, assess the extent of the basic scientific knowledge possessed by that readership, and then build upon that knowledge in the full understanding that it may be necessary to introduce some highly unfamiliar concepts and terms into the story. It is not necessary to think in terms of readers who have no interest in science: scientific matters have a lively interest to everyone with a normal sense of curiosity and wonder, although it is true that this sense can wither if the individual suffers a faulty education. Many people are inclined to underestimate the extent to which ordinary people who have never been inside a laboratory in their lives are interested in scientific facts and scientific ideas. The interest in astronomy, for example, is enormous and apparently insatiable, judging from the success which so many books about the stars achieve. On the other hand the science writer goes wrong, at least in my opinion, if he approaches the subject with the idea that every bright young person who reads his articles is a potential professional scientist. Such an approach is wrong because a community composed of nothing but scientifically-trained individuals would be an intolerable

abomination; it assumes that science is synonymous with omniscience, which is a false idea as all sensible scientists readily admit.

It is always pleasantly surprising to find how acute is the general interest in scientific matters. Indeed nowadays one frequently comes across this paradox: people who do not come into professional contact with scientists have a livelier interest in the general trend of scientific progress than many a narrow-minded scientist who earns his livelihood by the practice of science. An anecdote which illustrates this to perfection was told by Lord Moulton, the brilliantly versatile patent law expert who was appointed by the British Government chairman of the Dyestuffs Corporation of 1919, the organization which restored the fortunes of the British dye industry. Moulton met a solitary German on the top of a mountain.

> I found he was a chemist, and I began to talk upon a chemical subject. He told me he was only an organic chemist. He had not exhausted my resources, and I began to talk of coal-tar and pharmaceutical products. Then he told me he was a coal-tar by-product chemist. That did not beat me, because I had just been fighting a case of canary yellow. I thought I would get some subject that was common to us, and I slipped into the subject of canary yellow. Still the same ominous silence for a time, and then he said 'I am only coal-tar chemist dealing with blues.' But I had not finished. With an Englishman's pertinacity, not believing I was beaten, I racked my brains for a coal-tar blue—I had had to advise on some case—and I gradually, without a too obvious change of subject, slipped into that. Then he finally defeated me, because he said in equally solemn tones, but equally proud of the fact, '*I only deal with methyl blues.*'

Science ceases to be science, of course, once it is professionally compartmentalized to such an extent: a man who studies nothing but methyl blues is not a scientist but a machine, and it is to be hoped that such work will soon be taken over by machines. A more comforting thought, however, is the harmony between the interests of the layman and those of the broad-minded scientist. The ordinary person can be aroused to interest in most of the scientific items which can be made interesting to a 16-year-old schoolboy. It is this basic and universal interest on which the public relations of science must be constructed.

The increase in the amount of scientific jargon is one of the real difficulties; thus one finds, for example, that an individual scientist may be quite incapable of explaining to the science writer the particular research that he is engaged in. He relies on that jargon as a lame man on his crutch; he dare not throw it away, and yet so long as he depends upon it he cannot rid himself of the limp which characterizes his thinking processes. The situation is reminiscent of the occasion when Rutherford was president of the Royal Society and a young scientist was reading a paper to the assembled Fellows. Rutherford sat patiently listening to his flow of pure jargon, until eventually his patience was exhausted; he told the young scientist to throw away his paper and tell the story of his investigation in his own words. The young scientist's extempore exposition was a brilliant and lively success.

The use of jargon is however contrary to all the best traditions of science writing, and there are already hopeful signs that many scientists are working according to Rutherford's principle. This should bring great improvements in its train.

There are some who argue that there is one insuperable obstacle to science writing for the general public: the fundamental ideas of science are now so complicated that they cannot possibly be conveyed to the non-scientist. If this is correct, then how does one explain the fact that men like Einstein, Faraday, Helmholtz, Schrödinger and Prince Louis de Broglie have all taken the trouble to write books for the general public? If the public is unreachable and unteachable, surely such talented men would not waste their time in this way.

Admittedly the amount of accumulated knowledge is such that no one man can hope to grasp more than a minute fraction of it. One can look at any modern encyclopedia, such as the *Encyclopedia Britannica,* and despair of ever reading it right through and understanding each and every article in it—whereas in the eighteenth century a reasonably educated man could assimilate quite comfortably all of the French Encyclopedia if he had the time to read it.

The lesson of *The Origin of Species* is again encouraging in this specific context. This book contained a new theory of evolution, and it was solidly packed with unfamiliar details. Yet the book became a best-seller and reached far beyond the confines of the scientific world. Everyone with any pretension to being cultured read it, and many thoroughly grasped the main points of Darwin's theory, in spite of the fact that they were not easy points to grasp. This and the other instances quoted provide a fair rebuttal of the rather snobbish idea that the important concepts of science are beyond ordinary comprehension.

There is some substance in the argument that modern science cannot be explained perfectly without the use of mathematical formulae. But perfection in the sense of perfect scientific accuracy is not the aim of popular writing, so the solution is to leave out the formulae and to try to explain a fundamental new idea without them. Is it not true of most scientific research, to quote the words of Einstein, that 'no scientist thinks in formulae'? It should therefore be possible to put into words the steps in the thought processes which lead a scientist to formulate a new idea. Here the popular books of Einstein himself, Schrödinger and Prince Louis de Broglie demonstrate how it is possible to convey erudite and abstract ideas in simple language.

THE TECHNIQUE OF POPULARIZATION

The process of simplification may need to be done in a series of steps, not in a single jump. The concentrated character—the factual 'meatiness'—of

many original research papers makes a step-wise digestion process desirable and sometimes indispensable. Much scientific meat needs to be 'prepared' before it is suitable for general consumption. Thus a research paper published in, say, the *Proceedings of the Royal Society* first needs to be critically reviewed in a journal such as *Science Progress*. The next step in the process may well be the publication of a somewhat simpler account in a journal such as *Scientific American* or *Discovery*. At that stage, the daily papers will pick up the story and publish reports after putting the factual 'meat' through a further process of preparation. The final dish may prove a great deal more nutritious to the general reader than the scientist who wrote the original paper would ever have imagined. (Sometimes, of course, something goes wrong with this preparation process, and the result is a mess which would best have been thrown away.)

Popularization can rarely be achieved if it is regarded as a single-stage process. I believe this idea of a multi-stage process is one that can be exploited to the advantage of everyone concerned. It is a process, incidentally, in which the scientists obviously must play an important part, one of the stages in it depending on professional scientists and not on professional science writers. The process need not depend solely on the printed word. One stage could be performed in the lecture hall, for a good popular lecture (e.g. a Royal Institution Friday-night lecture) could be used directly or, after further preparation, by a daily paper. Radio talks may similarly find their way into print: thus Sir James Jeans produced his very successful book *The Stars in their Courses* by amplifying a series of BBC lectures on astronomy and adding pictures.

Some scientists feel that the only authoritative science writing is that produced by scientific workers. At the level of the daily newspapers, this idea is quite impracticable, apart from occasional contributions in the form of feature articles; it is impracticable, because of the time factor which necessitates the appointment of staff writers specializing in scientific news and its interpretation. Even at the level of book writing the idea is not thoroughly sound, and certainly it would be wrong to take the line that the writing of books about science should be a monopoly of practising professional research workers. There is one powerful reason why few books about science would get published if such a monopoly came into existence—the undoubted fact that the first-rate researcher is not likely to be much interested in this kind of writing. It may be argued that writing can be left to the second-raters, but the second-rate researcher may well turn out to be just as second-rate a writer and expositor. What Dr. C. D. Darlington said about research may equally well apply to science writing: 'Quality counts in scientific research; it is the *only* thing that counts. For what is not good is bad, and what is bad is a costly and dangerous liability.' Occasionally one comes across the first-rate scientist who also has that rare quality which the

Romans called *cacoëthes scribendi,* and which could perhaps best be translated by 'the itch to write'. But even then the itch to discover new things will generally prove stronger than his itch to write, and it is very exceptional nowadays for a first-class scientist with a flair for writing to produce anything more than research papers until after his research career is finished; it is hard enough to persuade such a man to write even a short article. The fierce competition of the modern scientific world does not allow him to indulge his literary talent to any significant extent. Such men usually take care to prevent that talent from going rusty, and then utilize it to great effect at the end of their research career; in that way some of the best scientific textbooks are written.

In our present and highly competitive society it is the specialist who rules the roost. Just as scientific research is the monopoly of the scientist, so writing is largely a monopoly of professional writers. The man who thinks he can combine the two activities is likely to make a poor showing at both. His prestige will be far less than he could gain by pouring all his energies either into one or the other. On the other hand, the scientist who has finished his research career and takes up writing as a more or less full-time occupation is, of course, in much the same position as the full-time professional writer.

The professional science writer has proved that he is indispensable to daily newspapers, at least in Britain and the U.S.A. Today almost all the national daily newspapers in Britain have a staff science writer or science editor, though only the *Daily Herald* and the *Manchester Guardian* covered science in this way before the war. In the U.S.A. coverage of science by leading papers such as *The New York Times* is excellent; most of the American city dailies have staff science writers and give ample space to science. The American press is also well served with scientific news by the big news agencies. Science reporting in *Time* is outstanding, and some of the finest and most beautifully illustrated popular articles on science are printed in its sister paper *Life*. It is significant that the latest Kalinga Prize—an international prize of £1,000 awarded annually by Unesco for distinguished career in science writing—went to Waldemar Kaempffert, science editor of *The New York Times*.

THE COMMON INTEREST OF SCIENCE AND THE PRESS

A fact which needs to be recognized by scientists who give any thought to science reporting in the daily press is that, in newspaper production, speed is the very essence, whereas in science the essential thing is accuracy—which means checking and counter-checking. There is room for compromise here; the scientist must realize that the set of facts which he has checked so carefully will be regarded by a newspaper as something completely dead by

tomorrow if a rival paper 'scoops' the story. 'There is nothing so dead as yesterday's newspaper' is an old Fleet Street saying. Hence the science reporter who checks and counter-checks will be indulging in a luxury that will make his paper last with the news, in which event the editor may be tempted to scrap him.

It is doubtful, too, whether scientists appreciate the relatively low proportion of erroneous reports which appear in a daily newspaper, in spite of the pace at which the work of news collection and publication is performed. Conscientious reporters will always admit their error when their stories are wrong in factual details—though it should be noticed that they may have been given the wrong facts by the people they interviewed in the first place. It is a pity that editors do not always print reasonable letters of correction sent in by scientists pointing out serious errors in published stories. It is true that many letters of correction that do arrive in newspaper offices are extremely rude and dictatorial, and richly deserve the fate of being pitched into the wastepaper basket. But it would be a distinct gain if editors could bring themselves to drop the pretence of infallibility, and print the reasonable corrections. But as Mr. Brimble remarked in his Edinburgh address:

> It seems they imagine they would lose caste, and perhaps custom by so doing. I believe on the other hand, provided it does not happen too often (and it does not), that the correction of an error, especially in science news, will inspire greater faith among readers. I shudder to think what would eventually happen to *Nature* if we refused to publish an erratum when one is necessary.

Scientists often make the mistake of being too finicky in this matter over some questions of fine detail; occasionally too they make an even worse mistake, and point out to editors errors which never existed at all, except in their imagination. (This is often the result of careless misreading.) The scientists need to accept the principle that it is the general impression that counts, and that it is more important for the general impression which a newspaper story creates to be sound than for every single tiny point of fact to be dead accurate. As one scientist has said: 'Details *should be* accurate, but it is unlikely that the public at the moment can appreciate or assimilate more than a very small proportion of them.' Factual errors therefore have a comparatively small chance to do harm, though obviously the science reporter will aim at reducing them to a minimum.

Too great insistence by scientists over trivial details can do a great deal of harm, for, paradoxically, it damages the conscientious science reporter more than anyone else. The really bad reporter will produce so ridiculous a story that a scientist will consider it beneath his contempt; he is inclined to direct his complaints against the very best science writers, whose work he reads rather carefully because he has a good deal of respect for it. Unfair complaints can be more than a little galling to the science writers with a conscience, whereas perfectly fair complaints would probably be ignored

anyway by the other kind of writer. As a result, quite a few good science writers have turned to other fields of writing where they are rewarded with less kicks and more halfpence; this is a great and serious loss to a tender young profession, which deserves to be given every encouragement if it is to reach maturity. It is within the power of scientists to kill this new profession of science writing, and the seriousness of this point should be fully appreciated; scientists, if they are willing to do so, can be extremely helpful, but they are also in a position to do more harm than good unless reasonable goodwill is maintained between the two sides.

One point with regard to factual accuracy in newspapers needs to be brought home with some force. Reporters are trained to aim at the highest standards of accuracy consistent with the speed of newspaper production. Errors of fact which get into print can lead to court actions and cause the expenditure of a great deal of money on lawyers, damages, or settlement out of court. An inaccurate reporter will not remain long with any respectable newspaper. The vested interest of the press in accuracy is one which can be registered in terms of hard cash, and that interest is both bigger and deeper than the more fanatical critics of the press are prepared to admit. Both the press and the world of science have therefore a common vested interest in factual accuracy; in both cases their prestige depends upon it.

The scientists and the man of the press have other interests in common. They both depend upon the printed word, and they both have a stake in the freedom of the press—a fundamental liberty which the men of letters carved out, and which was of immediate benefit to the early scientists. That freedom has been maintained over the past century largely through the courage and exertion of the journalists of the world. These are real and sufficient grounds on which to build that mutual respect between scientist and pressman that is today so much needed. Failure to do so and thus to improve the public relations of science is fraught with grave danger, for what is at stake is the opinion which the public forms about science.

Public feeling of a peculiarly bitter kind has been aroused by such recent developments as the atomic and hydrogen bombs. The public needs some reassurance that benefits that come from scientific research outweigh some of the horrors which science has made possible. Unless improved public relations can restore public confidence, the future of science does not look bright. It was Bertrand Russell who pointed out that but for his indispensability the scientist would today be persecuted all over the world. The public fears about science stem largely from public ignorance, and in the fight to dispel that ignorance the scientists and the press must become firm allies. Failure would affect the future of science itself.

ORGANIZING FOR APPLIED RESEARCH IN THE CIVILIAN FIELD

by

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INTRODUCTION

The requirements for effective applied research can be written down more easily than they can be realized. First, there should be a public climate favourable to research; the people generally should be interested in its achievements and in its possibilities for good. Parents should be keen, where their children show the aptitude, to see them take up science with a view to a career in teaching, research, technology or industry. Teaching facilities should be adequate, with an able and enthusiastic corps of well-remunerated teachers, both in the schools and the universities. There should be a sufficient volume of fundamental research sustained, as this is the head of steam on which applied research effort depends for its energy. The research clients —usually industry and government—should collaborate closely with the research workers both in setting up the research programmes and in carrying out the work so that the programmes reflect national or industrial needs, and so that the results are taken over smoothly and speedily for development and application. Adequate funds should be available for research and development; wherever possible, fiscal policies should be so designed as to give every incentive to industry to improve its processes and to invest in new production; and salaries of research workers at all levels should be attractive. Communication of research information should be so ordered as to enable the user not only to understand it but also to see how it affects his operations. The form in which research results are published should be compatible with the best industrial engineering practice; the research should, as far as possible, be carried to a stage at which industry can assess not only its technical possibilities but also its ultimate profitability. The workers should be made aware of the prospects which innovation opens up for increased production and prosperity, so that they will accept changes readily and operate them efficiently. Both industry and government should ensure that research is represented on their policy-forming boards or committees: unless policies are formed with a full appreciation of the possibilities of

research for new production or new markets, and unless boards of management and governments are research-minded, application will inevitably be slow and opportunities lost. Research is a long-term investment which seldom yields dividends to order. Support for research should not be spasmodic; programmes should be established so that there is a guarantee of continuity. Given such support and confidence, the dividends must follow unless the programme is ill-conceived or the selection and direction of personnel poor.

Few countries could claim to satisfy more than half of these important and numerous requirements, but most countries have their strong suits, and all are acutely conscious of their shortcomings. None is complacent.

There are many different types of research establishments in operation today. In most countries, private industry supports by far the greatest proportion of civilian research, with governments responsible for an increasing share. In many countries government, in addition to carrying out research in fields of public health and utilities, is actively encouraging industry to do more background research and to provide research services, particularly for the small firms. This encouragement usually takes the form of financial support for the formation of industrial co-operative research establishments, the greater part of whose income is derived from membership fees or, in some countries, from levies related in some way to the size, payroll or output of the individual member firms. Such co-operative concerns are particularly popular in Europe. In the United States of America, there is also a considerable volume of co-operative research—firms combining to support individual research projects, or research programmes sustained or financed by trade associations—but there is also an increasing number of private 'sponsored' research institutes which undertake research for industry on a confidential basis. Some of these operate for profit but a large number are non-profit concerns. It is significant that, although both the co-operative and sponsored type of research establishments provide research services for the small and medium size firms, both are strongly supported by the large industrial units. University departments, in addition to their valuable work in training research workers and sustaining programmes of fundamental research, also do a considerable amount of applied research. This is particularly true of the European technical high schools, institutes of technology in Europe and America, and colleges of engineering in the U.S.A. A very heavy programme is undertaken in and through American universities on government contract, and there is a feeling in some quarters there that the load may be in danger of becoming too heavy in view of the importance of the universities' other functions. It is also unfortunate that much of this government-sponsored research in universities tends to be 'classified', so that a large area of research is in danger of being sterilized for lack of free publication. This apart, however, the research contracts with universities can and do help to provide useful training for applied research workers and

give them an invaluable opportunity of getting to know more of the special requirements of their industrial or governmental client.

In addition to these main types of research establishment, there are, of course, a number of trade association laboratories, consultant laboratories and others which exist to satisfy industry's needs for research, testing, and information services.

These, in very brief general outline, are the main requirements and the tools for effective applied research. Let us see how they find expression at the present time both on the national plane and in the individual research establishments.

THE ROLE OF GOVERNMENT

There are many reasons why government responsibility for applied research has become increasingly important during the past 50 years. Two world wars have amply demonstrated the practical value of science, and important steps were taken during both towards organizing applied research in the civilian field on a national basis. As has been pointed out in the survey of National Research Councils for Pure and Applied Research published by Unesco,[1] the first of these councils were established in the United Kingdom, the United States of America and Canada in 1916. Since then there has been a steady increase in the number of such councils, which have varying degrees of responsibility for research policy and programmes but the same underlying responsibility for reviewing national needs in their assigned fields of science and, generally, for supervising the research programmes eventually drawn up.

At the end of the second world war the United Kingdom government established an Advisory Council on Scientific Policy (ACSP), to advise the Lord President of the Council in the exercise of his responsibility for the formulation and execution of government scientific policy, and thus created a permanent machinery for ensuring that the country's scientific resources would be effectively utilized in the national interest. The ACSP was given responsibility for surveying the whole civilian field of scientific policy.

In the U.S.A., also, the National Science Foundation (NSF) was set up in 1950 to develop a national policy for the promotion of basic research and education in the sciences; to support basic scientific research and to appraise the impact of research upon industrial development and the general welfare; to award scholarships, etc.; foster exchange of scientific information; and evaluate scientific research undertaken by federal agencies. The origin of the NSF is probably to be found, at least in part, in President Roosevelt's request to Dr. Vannevar Bush, in 1944, to study the nation's science needs:

1. See *Impact*, vol. IV, no. 4, Winter 1953, p. 231-55.

'New frontiers of the mind are before us, and if they are pioneered with the same vision, boldness and drive with which we have waged this war, we can create a fuller and more fruitful employment and a fuller and more fruitful life.'

The increasing complexity of science and the ever-widening field of research activity have rendered direct government action indispensable. In all countries there is at present a shortage of skilled research workers, and the only way to overcome it is to plan now for the training of an adequate flow of new recruits in the future. This operation must start with the schools, for without an adequate supply of science teachers, school buildings and laboratories there can be no lasting improvement. The same holds good for universities, which must be staffed and equipped to receive more science students and to train them not only in established disciplines but also in the newer technologies. New departments and new curricula add to the difficulties already facing the universities. The root of the problem of supplying more applied scientists and technologists lies, therefore, in the schools and universities, and there is no formula which overnight can provide bricks and mortar—far less provide skilled staff. The provision of educational and training facilities is generally largely dependent on government funds, and in this sphere alone the role of governments in applied research is paramount. As with all scarce commodities, it is essential to plan, as far as possible, for an output of skills from technical colleges and universities which will match closely the needs of industry, government and the research departments of the universities themselves. Too often one hears the statement that what is needed in Europe is more applied research and development, and less fundamental research. To adhere to this view would be shortsighted indeed: the volume of fundamental research must be maintained at a high level if the applied research is not to dry up. It is false economy to purchase immediate benefits at the expense of future poverty. Important studies of the needs for and supply of scientific manpower have been made in the U.S.A. by the Steelman Committee, and later by the Paley Committee; and in the United Kingdom by the Scientific Manpower Committee of the Advisory Council on Scientific Policy.

Scientific manpower is not the only scarce commodity today. Raw materials are also a problem, and here again governments have acquired important responsibilities. Even in the U.S.A. the rate at which industry is using the natural resources of that great country has stimulated the government to set up the Paley Committee, and the committee's report, *The Resources of Freedom,* has done much to underline the importance of long-term planning on a national basis as regards raw materials. In Europe, with its much older industrial history, the problem of materials is much more acute, and governments have been active in encouraging industry not only to conserve supplies by making more effective use of them and by eliminating

unnecessary waste but also, wherever possible, to recover valuable materials from waste products. The need to restrict imports from the dollar area has accentuated this problem. In the event, there has been a strong wave of interest in improved methods of locating valuable ore deposits, improved methods of ore dressing so that the maximum use may be made of lower quality deposits, and in sources of new raw materials such as the petrochemicals industry. Responsibility for action and commercial initiative still rests with industry, as is proper, but government is assuming more and more the task of general economic planning and national housekeeping, and it is doubtful whether we shall ever again see the days of plenty, with industry having a free run on the materials market.

Given the need to husband scarce resources in skilled manpower and materials and to economize foreign currency, government also has the overriding responsibility of so ordering its fiscal and financial policy that industry is given every incentive to invest in new equipment and in new production. In the latter activity particularly, and in the development and semi-technical phases which precede the decision to go into production, capital costs are heavy and the risks are severe. Every encouragement should obviously be given to industry to branch out wherever it would be fruitful to do so. This problem is particularly pressing in Europe where old industries require modernization and where there is so much physical damage to repair.

Government departments themselves also undertake research or commission research on contract with industry and university. Their research establishments usually function in the fields of community services and in the provision of general background research of value to industry as a whole. Development is normally the responsibility of industry, though in some instances, notably the aircraft industry, government may largely bear the initial financial burden. In some countries it is felt that governments should bear more of this burden in an attempt to accelerate the rate of innovation in industry, and a recommendation of the U.K. Advisory Council on Scientific Policy on these lines is being given serious consideration.[1] There are certainly important areas of research which give no immediate prospect of commercial profitability but which show a long-term promise of real advantage on the national plane, and in these fields, at least, government intervention would appear to be amply justified.

In Canada and the United Kingdom, government has also tried to ensure that research results arising from work paid for by public funds are not allowed to languish if they have any potential value. In both countries, national corporations have been set up with funds to invest in such development—the National Research and Development Corporation in the U.K., and Canadian Patents and Development Limited in Canada.

1. *Sixth Annual Report of the Advisory Council on Scientific Policy (1952-53)*, H.M.S.O., London.

Government research establishments are liable to suffer somewhat from the fact that they must operate on the basis of annual Treasury approvals of the research budget. This can be extremely cumbersome and makes long-term planning difficult. There are also felt to be complications due to the civil service system, making for a certain rigidity in the organization. That these can be overcome is evidenced by the recent announcement in the British Parliament that the Department of Scientific and Industrial Research has been authorized to plan its programmes and resources on the basis of a five-year period (subject, of course, to annual parliamentary approval);[1] and by the National Research Council of Canada which, though its funds are voted by Parliament, appoints its own staff without government control and can earn income by doing sponsored work.

The interest of government in applied science is not altogether altruistic. Since the second world war there has been a new development in the appointment of government scientific officers with the responsibility of advising on the application of research results generally, and on the implications of science for government and departmental policies in particular. The United Kingdom set the fashion when, during the war, the Prime Minister appointed an official personal scientific adviser; and, more recently, on the recommendation of the Advisory Council on Scientific Policy, scientific advisers have been appointed to a number of government departments. In the U.S.A., a scientific adviser has been appointed to the State Department to advise the Secretary of State on scientific questions bearing on international relations. So science is being given the opportunity to have a say in government affairs. And not only ministers are so served; the Parliamentary and Scientific Committee of Great Britain is unique of its kind and does valuable work in keeping members of Parliament informed of the role which science can play over a wide range of public affairs.[2]

THE ROLE OF INDUSTRY

Industry is responsible for by far the largest volume of applied research in all countries, though the distribution of the research effort is uneven both as between industries and within industries. The newer industries, such as the chemical, the aeronautical, the electrical, were conceived in science and maintain a strong research effort. Older, more traditional, craft-based industries have been slower to realize the benefits which can accrue to them from research, but here too there are indications that the importance of applied research is now more clearly understood. Within industry, the bulk of the professional scientific manpower is employed in the larger firms.

1. *Department of Scientific and Industrial Research, Report for the year 1952/53*, H.M.S.O., London.
2. See *Impact*, vol. III, no. 4, Winter 1952, p. 258-76.

Research in industry covers the whole spectrum from fundamental work to pilot plant operation, production control and trouble-shooting. Differing views have been expressed on the value of fundamental work in industrial laboratories. The view prevailing in the best of the large companies is that a strong core of fundamental research is the surest way of attracting those first-class scientific recruits without whom the general level of research and development will deteriorate, ultimately harming production. The main emphasis is, however, on applied research and development, and industry is there infinitely better placed than government to see results through to application, as there is continuity of responsibility for all phases of work, and the research teams have at their disposal the services of development, production and sales departments alike. Obviously, if a firm is investing money in research and development work, it will want to exploit the results in the form of improved or new production. It must also be stimulating for the research workers to see their results considered on their merits and, when approved, or when company policy adjudges the market favourable, to see them utilized by the production departments. There can, of course, be frustration even in the large companies, if only because it may not be possible, or commercially tactical, to capitalize all the potentially useful results.

In industry, even more than in government, it is essential that management and those responsible for company policy should not only be informed of the research possibilities but also be sympathetic to research in general. The research director should have ready access to the policy-forming body, if indeed he is not a member of it, so that he can advise on matters of future policy and be fully informed as to the company's policy at any given time. There appears to be a more general appreciation of the importance of having research vice-presidents in industrial firms in the U.S.A., where the combination of research and business management is more effective than in Europe. In the U.S.A. there also appears to be a higher proportion of scientifically- or technologically-trained men in industrial management than is common elsewhere, and this is believed to be a strong contributory factor to the high speed of application of new methods and processes obtaining in that country.

In times of scarcity of scientific manpower, as is the case at present, it is very important that what is available be used to the best advantage in industry. There is little reliable information published on this point. In some countries, there are indications that purely economic considerations, such as salary scales and general financial prospects, too often dictate the work a science graduate takes up in industry regardless of his special aptitudes or personal preferences. Some degree of salary differentiation is probably inevitable, owing to the fact that the freedom allowed to the individual is much greater in research than in production, but it should, as far as possible,

strike a fair balance. The National Research Council of Canada has set a good example by undertaking a survey of professional salaries with a view to striking a fair balance within the whole structure of Canadian science.

When one considers how carefully industrial firms scrutinize their trading accounts and seek to control their production costs, it is surprising to discover that very few of them appear to have been systematic in their study of research investment and its relation either to the needs of the company or to the value of the results achieved. Undoubtedly most firms operating a research section must have considered at fairly regular stages whether it was worth while continuing with items on the research and development programmes, and it is probably true that technical-economic scrutiny is part and parcel of the normal cost accounting process; but there is no published body of information available on this subject. It is clearly desirable to know more about the level of research investment normally necessary to maintain a given level of production or a given rate of innovation. Case histories of this sort, giving reasons for decisions to stop or to proceed, would be of immense value to anyone interested in the planning of research and the formulation of research programmes and budgets. At the present time a growing number of firms in industry are adopting what is now known as 'project evaluation', and production economics or technical economics is being increasingly recognized as a fruitful field for research. There is thus every prospect of more systematic data of this sort being available in the future.

Though the large firms in industry sustain a great volume of applied research, few if any could afford, or would wish to try, to satisfy all their research needs in their own laboratories. The tendency is for the larger units in industry to concentrate primarily on those research fields which show the greatest promise of commercial advantage. Generally speaking, the ability to develop discoveries within their own laboratories is a feature only of the large concerns; smaller firms lacking their own research and development facilities must find some other means, and two most interesting types of industrial research establishments have grown up to meet their requirements—the co-operative research associations and the sponsored research institutes.

CO-OPERATIVE RESEARCH

Manufacturing industry in Western Europe contains a high proportion of small units, and it is interesting to note how most countries there have resorted to co-operative research establishments as a means of ensuring that at least the basic research needs of the industries are not overlooked. Whether the initial motive was economy of scarce scientific manpower, lower research

costs for industrial members through cost-sharing, or the desire to raise the general level of technology in the industry concerned, the co-operative research movement has grown and spread—with certain national variations in form.

In *Belgium,* about twenty new co-operative research groups have been formed on the initiative of the Institute for the Promotion of Scientific Research in Industry and Agriculture (IRSIA),[1] in addition to the Centres de Groot which in many ways resemble the French co-operative research centres. Membership of the latter is obligatory and the income is derived from (a) a single state grant at the time of foundation and (b) an annual levy paid by all firms in the industry served.

In *Denmark,* nine institutes have been established under the Academy of Technical Sciences. Their incomes are provided by industrial associations and, in most instances, are supplemented by a government grant. The Textile Research Institute is worthy of separate mention as it was set up by a fund collected once for all from industry by a levy equal to 1 per cent of the wages. This fund continues to provide the bulk of the institute's income, though government subsidizes the purchase of apparatus.

In *France,* there are no fewer than 75 important centres carrying out co-operative research. Their legal form and methods of financing vary widely. They may, in some cases, accept contract work on a confidential basis against repayment, but all are financed by levies on industry, some voluntary and others compulsory. The services provided vary with the requirement of the industry served, and range from fundamental research to advisory and training functions.

In *Germany*, the classical method of research co-operation is embodied in the Max Planck institutes. In recent years, however, there have been several small groups formed to promote co-operative research, usually by placing work in university or other laboratories. The largest co-operative programme is that initiated by the Verein Deutsher Eisenhüttenleute (the equivalent in size of the British Iron and Steel Research Association or the French Institut de Sidérurgie). Such co-operative work is usually subsidized by government.

In the *Netherlands,* co-operative research is normally based on the TNO institutes,[1] on the research councils of which there are representatives of industry and government as well as independent scientists. Government pays for the free research work, and industry for the contract or sponsored research done for individual firms or groups of firms, so that the institutes are really a combination of government research establishment and sponsored research institute. In view of the valuable work of the TNO institutes, a number of research associations have recently been set up, organized on lines similar to those met in the United Kingdom.

1. See *Impact,* vol. IV, no. 4, Winter 1953, p. 233, 251.

In the *United Kingdom,* there are about forty industrial research associations, all independent of government control though in receipt of government grants. Membership of the research associations is voluntary and, though not expressly forbidden, little if any confidential contract work is undertaken.

In *Norway* and *Sweden,* there are further interesting variants in addition to the normal types of co-operative establishments. In Sweden, there are a few fields in which, by formal contract, government provides buildings, certain services and salaries of staff employed on basic research, while industry provides instruments, running expenses and salaries of staff on applied research. Another interesting research organization in Sweden is the Research Station of the Academy of Engineering Sciences (IVA). This station is maintained by the academy and, by individual agreement, rooms, equipment and services are placed at the disposal of various research groups for definite programmes, generally for limited periods of a few years. Each group has its own staff and finances its own work. There is no rent, but a service charge is made covering 50 per cent of the actual cost, the rest being met by a state grant. In Norway, mention must be made of the Central Institute for Industrial Research which has been set up by the Royal Norwegian Council for Scientific and Industrial Research and which adds to the sort of services provided by the IVA research station an industrial sponsored research service.

In *Canada,* also, though there are no independent co-operative research associations, the Associate Committees of the National Research Council and the 'fellowship laboratories' of the Ontario Research Foundation provide common services to firms in industry.

There is much to commend the Norwegian and Swedish plan of providing a central establishment which can be used for specific projects in times of short supply of research workers. It may be that there is a limit to the number of separate industrial research establishments any country can afford to sustain, even allowing for the manpower economies achieved through co-operation, and that marginal or periodical requirements should be catered for centrally, as they are in Scandinavia.

SPONSORED RESEARCH

While the co-operative research associations were growing in Europe, there was a corresponding movement in the U.S.A. to provide research services for industry. In this instance, however, the emphasis was on the provision of facilities for confidential sponsored work, as befitted a country particularly devoted to private enterprise. The first of these sponsored research institutes was the Mellon Institute, followed by the Battelle Memorial Institute and the Armour Research Foundation. The number of such institutes and the

volumes of research work they handle has grown rapidly, particularly since the last war. An examination of their income from research shows that at least half of it is being provided by the government in the form of contracts, though this proportion would be reduced drastically if the U.S. Government decided to cut down its very large current research expenditure.

There are a few sponsored research institutes in Europe which correspond closely to the American model (except in volume of business), but the existence of so many firmly-established co-operative research associations makes it very difficult to know whether there is a real demand for a sponsored service in Europe, or whether the market has been cornered by the co-operatives. The impression gained on visiting the American institutes and discussing their operations is that they are extraordinarily positive in their approach to research and that they are extremely skilful at selling their wares. As they depend on contracts for their continued healthy existence, they appear to be almost more businesslike and certainly more cost-conscious than their industrial research colleagues. The result is an almost percussive approach to industry, with the research man accepting the responsibility for surveying regional or individual research requirements and development possibilities; and, on the basis of his survey, lining up research projects and explaining to the potential client why it is in his interest to pay for some research work. The Battelle Memorial Institute has recently established a European counterpart known as Battelle International with branches in Switzerland and Germany. The laboratory at Frankfurt has been established with remarkable speed, research work being started within two years of the first decision being made at Columbus, Ohio. This European venture is being financed initially by the American institute but is expected to become self-supporting. There are indications that Battelle International will extend its activities to other countries and that other American-sponsored research institutes may also open up branches in Europe, and it will be interesting to see whether the American approach will succeed as well in Europe as it has done in the U.S.A.

One or two similarities between the co-operative and sponsored type of research establishment are probably worth re-stating:

1. Both exist to serve industry, particularly the smaller units in industry.
2. Both are substantially supported by government, the one by grants and the other by contracts.
3. Both are strongly supported by, and probably derive the major part of their industrial income from, the large firms.
4. Neither is a substitute for research in industry's own laboratories. Both are complementary to it, sponsored research claiming to make individual firms more competitive through its confidential contracts, co-operative research claiming to raise the general level of its industry and so improve the competitive position of the industry as a whole.

There appear to be good grounds for believing that the two systems can be compatible with one another, and it may well be that the composite organization will in the long run prove to be the blueprint for the future. Here, as in considering the national research arrangements, the question of balance between the fundamental, long-term and *ad hoc* is vital, and there can be no generalization. Each director of research must fix the point of balance best suited to the needs of his industry or his clients.

FORMULATING AND ADMINISTERING PROGRAMMES

Unless research and development programmes are well conceived both at the national and the individual laboratory level, progress will be fractional. However well conceived the programme, research cannot succeed without an adequate system of internal organization.

In the formulation of research programmes the research director is usually advised by a research council, board or committee. In industry, he can draw on the experience and *expertise* of the other departments, particularly on production, planning, finance and sales; he usually also has close working contacts with related university departments and can call on professors and other academic research workers as consultants. In government, the general policy may be considered by advisory councils (on the lines of the U.K. Advisory Council on Scientific Policy) or by the appropriate national research councils. In the United Kingdom and throughout the British Commonwealth generally, such advisory bodies are responsible to a senior minister of the Crown who has no responsibility for a large department. Their membership consists of industrial and academic scientists and, though the research is financed mainly or entirely by government funds, long-term policies and programmes can be evolved without interference on departmental or political grounds. There is also sufficient independence of action to enable a degree of flexibility in planning and a reasonable balance between long- and short-term projects. Such flexibility and independence are essential if the research organization is to adapt itself easily to changing conditions.

The research director is himself responsible, in all areas of research activity, for ensuring that his programme is sound both in its science and in its practical usefulness. The advice of his board and the suggestions of his scientists must in many instances be supplemented by technical and economic surveys of the conditions in which the industry or the research client concerned operates. Such surveys are being increasingly recognized as an essential prerequisite of good programme planning, and it is interesting to note how they are growing in number and importance. The economist, scientist, engineer and sociologist are beginning to realize how closely their activities are interconnected in industrial concerns and how interdependent

are the factors affecting individual and collective effort and efficiency at all levels, from the shop floor to the board room. Programmes of operational research are increasingly being initiated with a view to identifying such factors and assessing their relative importance. They also simplify the subsequent evaluation of many of the research results which have led to major or minor innovations. This study of industry's actual operations by a team combining several related specialties has only gained momentum since the second world war, and much fundamental work, particularly in the economic and social sphere, remains to be done. The possibilities are considerable.

If his programme is to be productive, the research director must have an internal organization which ensures a reasonable degree of flexibility and freedom for the scientific staff. While it is true that outstanding individuals are responsible for many of the more important discoveries, the majority of research workers are concerned with investigation, measurement and fact-finding. As the laboratory grows, the pattern of organization clearly becomes more formal and the problem of ensuring adequate communication and integration more difficult. It is important, therefore, that the administration of the laboratory should be designed so as to make communication easier, and not more difficult.

Scientists in large laboratories are grouped in various ways, e.g. according to branches of science or technology (physics, chemistry, metallurgy, etc.) or to fields of investigations or stages of research (fundamental research, applied research and development). In general, government research establishments tend to organize themselves according to branches of science and, as they are mainly concerned with fundamental and applied research, research workers enjoy a considerable amount of freedom. However, where there is a strong emphasis on development work, as in industry, there must be closer co-ordination between the various research groups so as to ensure an easy transition to application and to co-operation with the production departments. Technical control is also more direct than in the research laboratory. There is no blue-print for internal research organization, which must vary with the size of the unit and with the stages of research with which the unit is concerned. But once the objectives of the research programme are known, the essential prerequisites for success would appear to be the following: research personnel chosen for their particular aptitudes in relation to the programme; an adequate system of communication both between research workers and between the research director, heads of section and research workers; co-ordination of the programme as a whole and, more specifically, of work related to a particular objective; machinery for periodic review so that progress may be assessed, tasks re-assigned, guidance given to the research workers and, whenever appropriate, decisions taken either to continue the work, to abandon it or to commence new work. J. A. Leermakers, of the Eastman Kodak Co., has summarized the general requirements thus: 'No

industrial research laboratory can be successful unless it has an effective research director, a first-rate scientific staff, an organization favourable for the work of the laboratory, and some system for guiding the scientific work. In the final analysis, it is the individual scientist who produces the results of value to the laboratory, and he must be provided with the physical and mental environment most conducive to stimulating and assisting him in his work. The successful laboratory is one in which an effective system of co-ordination ensures that technical ideas and inventions are directed towards results of value to the laboratory.'[1]

These conditions differ fundamentally from those in a production department, but there is a trend towards using in the actual ordering of research programmes more of the methods which management is using in productive industry. Cost control and project evaluation techniques are already fairly widely applied in those research fields where commercial application is the prime objective, and in the sponsored research organizations where the programme is largely compounded of projects done under contract. It is not unreasonable that the research manager should himself employ the methods evolved by his fellow scientists and technologists for industrial management, though care must always be taken to apply them only where they make the research organization more efficient, and not as a means of regimenting or controlling the creative research worker.

COMMUNICATION OF RESEARCH INFORMATION

As science and technology become more specialized, as the volume of applied research and development throughout the world increases each year and industry becomes more adept at applying research results, the problems of communication become vastly more difficult. Merely to multiply the number of publications is no solution, as the volume already in existence defies the efforts of the average individual to cope with it. The problem becomes the more serious when one considers to how many different industrial fields any research result may now be relevant. Just as line-management is not appropriate to research organization, so simple line-communication is quite inadequate for the dissemination of research information. Then again, with the exception of the very large industrial concerns, industry is not generally well organized to receive, sift and assimilate all the available research and technical information which may be of direct value to its manufacturing operations. This is particularly true of small-unit industry, which predominates in most countries. There are therefore several levels at which this communication problem must be considered.

1. J. A. Leermakers, paper read at the Second Annual Conference on Industrial Research, Columbia University, New York, June 1951.

Taking research establishments only, it should be the function of management to ensure, as far as possible, an adequate and regular exchange of information and experience within each establishment. There still remains, however, the problem of ensuring that as much information as possible is exchanged with other research groups, not only in the interests of economy and of avoiding duplication, but also because of the great advantages of cross-fertilization in the research field. Scientists usually keep themselves well informed on publications in their own special sector. In many instances, also, they have the opportunity to meet and exchange views at national and international conferences. Reliance on publications, however, can involve considerable delays and, if the current tendency of some governments to 'classify' a large proportion of applied research continues, the proportion of work eventually published will decrease. Conferences also show a tendency to grow in size and to consume more time than the result may justify. Even within what might be called the 'free' area of applied research there is room for improvement in the provision of information on work in progress (and on work abandoned which can in many ways be just as important). The extent to which industrial research establishments release information and results varies in accordance with the policy of the company, some waiting until they have patent cover, while others release information remarkably freely, relying on their ability to capitalize on their research results quicker than their competitors.

When we consider how to get research and technical information most effectively to the user, we are up against a much more difficult problem. How, first of all, to make contact with him? And having made contact, how to make reasonably sure (a) that he can understand it; (b) that, having understood it, he can see how to make use of it? More often than not the small firm literally wants spoon-feeding.

In all countries the technical press, trade associations, research associations and large supplying firms make a considerable effort in the provision of technical information services. Governments, too, through their research establishments, provide much direct service, particularly in the field of agriculture, but also in the publication of abstracts and technical digests. The volume of paper devoted to this information service is immense and its very bulk presents a major research problem, since few industrial firms can afford the organization necessary to digest it. As one would expect, the provision of a technical information service and the maintenance of an industrial liaison service are strong features of the co-operative research associations. Most of these associations devote a considerable amount of time to visiting member firms and discussing on the spot not only the implications of research work in progress and the way it could be applied, but also the problems confronting the firm and the way they may be overcome by using the research and testing service of the research association.

In many instances, the industrial liaison officer may have to tell the smaller units what their problems are, and it is obviously advantageous that he himself should refer the problem to the laboratory as he can specify it in a form which is familiar to the scientist. The co-operatives also carry out much of their research and development work on the premises of member firms and undertake training programmes for technicians. All this work creates a more intimate understanding between the industrial unit and the research personnel serving it. By familiarizing firms which have not got a research section of their own with the possibilities of research, the general level of industrial research-mindedness is increased; and, by participating in the earlier stages of research and development, firms acquire very valuable 'know-how' and are therefore more likely to accept and apply research results eagerly. Unfortunately, it is not possible to serve every firm in industry in this way, even on a co-operative basis. But it is to be hoped that industries not yet adequately serviced will progressively become aware of the importance of research and either equip themselves for the job or support some other organization which is prepared to do it for them.

Industrial liaison is not, of course, a monopoly of the co-operative research organization. Government research establishments, sponsored research institutes, trade associations and certain large firms also provide services of this sort in their various fields. An important aspect of this liaison work and of the increasing amount of operational research and other work done in partnership with industry is the increasing knowledge gained by the research worker both of the needs of industry and of the conditions which his results must fulfil if they are to be applied efficiently. This feedback must have invaluable results in the long run, not only because of its effect on research programmes but also because of the insight it gives the scientist into the needs of industry and the general field of industrial engineering and industrial economics.

As an illustration of national initiative in the industrial liaison field, reference should be made to the technical information services operated in Canada by the National Research Council and the Ontario Research Foundation.

Another interesting development is to be found in Sweden, where hundreds of specialists in various branches of science and technology have agreed to act as local or regional focal points for the review, selection and selective dissemination of information provided centrally by the library of the Royal Swedish Academy of Engineering Sciences. The service is provided free and the 'contact men' are content to act solely for the purpose of promoting the use of science in Sweden. They regard their appointment as an honour and as sufficient recompense in itself.

In the international sphere, there is also much activity both in exchange of information and in active collaboration in applied research and develop-

ment. This has been reviewed by Dr. A. King,[1] and it is only necessary here to underline his major conclusion, i.e. that international collaboration is extremely valuable for projects which are appropriate for international effort and which show promise of real advantages over normal national or institutional means. The recommendations of the British Commonwealth Scientific Conference, held in Australia in 1952,[2] leave no doubt as to the importance placed by the conference on international collaboration in the field of information services and on all activities directed to the application of the results of research in industry. In their opinion, while economic, political and fiscal questions have considerable bearing on the matter of productivity, technological improvement or innovation is the most important single factor without which nothing much is likely to be achieved.

CONCLUSION

It is abundantly clear that the importance of applied research and development in the civilian field is now universally recognized, both as a means of improving general standards of life through better amenities and as a major factor in determining the efficiency and profitability of industry. Generally speaking, all countries, both old and young, technically-advanced and backward, have problems to overcome before they can claim to be efficiently organized for applied science; and it is not necessarily an advantage to be too long established in science, as traditional patterns are often more difficult to adjust than are new ones to introduce. Nor does it follow that the country which produces the best concept of research organization is necessarily most adept at putting it into effect. With the rapidly changing research and industrial scene of today, flexibility is an indispensable feature of national and industrial research organization; but flexibility may be difficult to maintain if the research organization becomes too big and too cumbersome in its administration. Young industrial nations are fortunate indeed to have such a wealth of working examples to guide them in their choice of the organizational pattern most appropriate to their economy, examples ranging from the old and mature nations of Western Europe to the young and vigorous economies of North America.

1. See *Impact*, vol. IV, no. 4, Winter 1953, p. 189-220.
2. British Commonwealth Scientific Conference, *Report of Proceedings*, H.M.S.O., London.

BOOKS AND PUBLICATIONS

Scientific Calvinism

C. D. Darlington, *The Facts of Life,* Allen and Unwin, London, 1953, 455 p., 35s.

Professor Darlington himself relates that when one of his friends asked for *The Facts of Life* at a well-known London bookseller's, the assistant raised his eyebrows and replied, 'Sir, we do not stock that kind of book.' Although the title is ambiguous, the scope of the book is clearly indicated in the preface:

> The past fifty years have seen, after centuries of fruitless endeavour, the discovery of the processes of reproduction and heredity. These are ordinarily known as the facts of life. . . . All aspects of life are bound up with these central notions of reproduction and heredity. In one direction they have led us to understand evolution, in another development, infection and disease, and in yet another, race, class, mating and the development of society. And finally our inquiry reveals something of the scientific method, of the unity of knowledge and of the interpretation of history, including the history of the fallacies and superstitions with which we are brought up. . . . My serious purpose is to show the immense possibilities which await the application of the elementary principles of heredity (or, as we call it, genetics) to the great problems of society—of education, of medicine, of crime and punishment, of marriage and divorce, of the relations of races and classes, and, taking the long view, of evolution. When these possibilities are clear the way will be open for applying the methods and notions of science for our social well-being or, at least, not taking too much for granted, for our survival.

The central theme which is intended to unify such a diversity of material is genetic determinism. The first half of the book is concerned with tracing the historical emergence of this concept to the highly-developed and all-embracing form expounded by the author. In the second half of the book the concept of genetic determinism is applied to social, ethical and other problems concerning individuals and societies.

Darlington begins by outlining briefly the curious ideas held by the learned men of previous centuries on the subject of heredity. The mixture of penetrating observation and preconceived philosophy of the ancients, the polemics between the spermists and the ovists and the advocates of preformation and epigenesis, and the controversy concerning spontaneous generation are all described in a lively and entertaining style. Some attempt is made to explain the origins of these views, most of which are of historical interest only. Nevertheless, it seems that this part of the book suffers from the defect common to almost all histories of science: there is little sense of period or of the circumstances or intellectual *milieu* in which Aristotle or St. Augustine or even Leeuwenhoek lived and worked. It is like reading a detective novel where not only is the murderer known from the start but all the false clues are palpably unconvincing. We are of course obliged to consider the inter-

pretation of our predecessors *post hoc,* but that does not mean that we must always examine them completely out of context. Thus, Aristotle's assertion that in heredity the female provides the matter while the male provides the motion can hardly be considered apart from his general views on natural history and cosmology. As we read *The Facts of Life* (or almost any other publication on the history of science), we cannot but wonder that such brilliant men should have been so credulous. But while we are reading the writings of Aristotle, Leeuwenhoek, Spallanzani and the rest, we realize how penetrating and consistent they were, and how great an effort was necessary to dislodge their principles and look at the same natural phenomena in a different way.

Since Darlington is chiefly known for his important studies on the chromosomes of plants, it is not surprising that he comes into his own when describing the three great trains of biological thought that developed independently during the nineteenth century and fused to give us our modern views of heredity early in this century. These are the theory of evolution, the advancement of our knowledge concerning the structure and behaviour of individual cells, and the Mendelian concepts of particulate inheritance and segregation. In relation to the theory of natural selection, Darwin had to deduce certain facts about heredity. Once the belief in the fixity of species had been abandoned, it was necessary to assume that hereditary differences occurred in wild species, as they normally do in domesticated forms. Nature would then eliminate some of the forms and select the others for breeding, so that the character of the race would slowly change. The nature of heritable variation is therefore of great import for evolution, but it was only imperfectly understood by Darwin. The ideas of the struggle for existence, the survival of the fittest and the descent of man took the centre of the stage during the latter part of the nineteenth century. Heredity was seen entirely in the light of the evolutionary theory of Darwin, Huxley and Herbert Spencer: that is to say, as Johanssen put it, in a very deep shadow. Small wonder that the direct experiments of Mendel on inheritance in plants, neatly contrived and statistically analysed, failed for several decades to attract general notice. But the Mendelian segregation of genetic determinants could ultimately be correlated with the behaviour of the chromosomes. We now know that the great majority of hereditary characters are transmitted in the form of nucleoprotein particles whose segregation, crossing-over, and so forth depend upon the mechanics of the chromosomes in the sex cells. Once the door to understanding was opened, the rest could follow: distinct genetic changes or mutations were observed to occur spontaneously, and to be increased by irradiation and other physico-chemical agencies. And it has come to be realized that in mutation and genetic recombination every living species possesses a great deal of potential variation upon which natural selection can operate effectively.

However, the problem of continuous individual variation within a species gave rise to difficulty. Galton discovered that when certain variable characters are examined statistically they show properties of great interest. If the heights of Englishmen, for example, were arranged in different classes, it could be shown that they were most numerous at 5ft. 8in. and less numerous at higher and lower classes. The data when plotted give a 'frequency distribution' curve which is now applied to many continuously distributed characters. Soon after the publication of Galton's *Natural Inheritance* in

1889, a school of biometricians grew up believing in the natural selection of continuously varying characters. On the other hand, Bateson, who was the chief Mendelian of this period, deliberately chose constantly and sharply differing variations for study. Battle was joined between the two camps about the mode of inheritance of natural variations. The difference was resolved only when it was shown that many small genes or 'polygenes', each of small effect, could govern the size of an organ or a number of organs, so that the end result would appear as a continuous variation. In particular situations, where a constant environment can be maintained, polygenic systems controlling small quantitative variations can be exposed and examined by suitable breeding experiments. Mather was able to establish the importance of a polygenic system in determining the number of bristles in the fruit fly *Drosophila,* for instance. But it should be recognized that, under less rigorously controlled conditions, the distinction between polygenic inheritance and environmental influence is blurred and very difficult to define. This is the situation relevant to such important continuously varying characters as human physique and intelligence, which will be considered shortly. Incidentally, there is good material in the history of genetics for a Marxist interpretation: tier upon tier of thesis, antithesis and synthesis at a higher level.

All these important discoveries are now part of the body of orthodox biological science, in the greater part of the educated world at least. We take them so much for granted that we seldom feel the same exhilaration about their success that Darlington, who was himself in at the kill, can express: 'The final revelation of the chromosome theory begins to appear as one of the great moments in the history of human achievement . . . here was, in fact, for the first time, a generalization in biology on a footing with the generalizations of physics and chemistry.' It may also be true, as Darlington passionately states, that 'The immense implications of Mendel's assertion, first for biology, and then for human thought as a whole, have not yet seeped through in the intervening ninety years.' Mendel's concept of 'elements which determine' was at first cautiously and rigorously derived from carefully-designed experiments. It is a far cry from his conclusions to Darlington's sweeping claims for genetic determinism: the reactions of the living individual with any set of outside events 'are decided before they occur and can be predicted'. Small wonder that a soldier, when listening to Bateson lecturing to the troops in 1917 (before Bateson himself realized whither his views would eventually lead), cried out, 'This is scientific Calvinism.'

Consideration of the significance of genetic determinism is facilitated by the use of certain technical terms. For the elements in the fertilized egg by which the characters in the organism are determined Johanssen introduced the term 'gene'. To him also we owe the valuable terms 'genotype', by which is meant the genetic constitution of a plant or animal which is passed on from generation to generation, and 'phenotype', by which is meant the actual constitution and appearance of any plant or animal. It is a cardinal principle of Mendelian genetics that the genotype is uniquely determined, that is, it depends upon a particular combination of genetic characters and will be changed only by mutation. The Michurin-Lysenko heresy is chiefly concerned with alteration of the genotype by external agents. Darlington devotes a chapter to this concept and the evidence adduced in support of it, which

he regards as fraudulent. In any case, there are recent hopeful indications of a return to Mendelian orthodoxy in the U.S.S.R.

But while the genotype is uniquely determined, the phenotype is jointly determined; it is the result of interaction between genotype and environment. The central problem is, when given a particular phenotype, to ascertain to what extent it is the result of hereditary characters and to what extent of environmental influences. Numerous experiments could be quoted to illustrate the effect of environment upon certain simple genetically determined characters. Thus, an abnormal gene has been observed in the fruit fly *Drosophila* which causes a deformation of the hinder part of the body. Flies which are pure for this mutation regularly exhibit the deformation, which is termed 'abnormal abdomen', if they are grown in moist cultures. They are, however, perfectly normal if grown in dried cultures. In fowls there is a single gene responsible for the so-called 'frizzle' type which has curled and defective feathers. The 'frizzle' chick normally remains almost bare during the first year of life, but if it is protected from heat loss by enclosure in a woollen jacket it will develop an almost complete plumage over the whole body within three weeks. Again, the appearance of black pigment in Himalayan rabbits and Siamese cats (which is genetically determined) occurs only below certain critical skin temperatures.

Clearly, therefore, the environment can influence the appearance of certain simple and easily analysed genetic characters. There is every reason to suppose that the genotype is also influenced by environment in the case of complex or polygenic characters such as human physique and intelligence. Thus, Shapiro[1] made a detailed comparison between a group of Japanese in Hawaii, their relatives who remained in Japan, and their offspring who were brought up in Hawaii. It was found that growing up in the new environmental conditions had produced marked differences between the Hawaiian-reared Japanese offspring and their Japanese-born parents and relatives. From these and other observations it is certain that human physical characters are affected by nutrition and other environmental factors.

The same type of interaction between heredity and environment can be shown to occur in the case of intelligence. That there is a strong genetically determined component of intelligence is now well-established. It is also evident that heredity plays an important part in the development of particular faculties in animals. Hall was able to breed strains of rats that were very much more efficient at maze running than ordinary laboratory rats. And all the studies so far made of human intelligence, using a great variety of tests, have shown a distinct positive correlation between parents and offspring, and between brothers and sisters. But the same tests show the influence of environment on intelligence scores no less clearly. When adopted children are tested, the scores are at first closely correlated with those of their relatives at home and poorly correlated with those of the foster parents; later the correlation with the relatives decreases and a positive correlation with the foster parents develops. Young children of bargees have nearly normal intelligence scores; later, since these children receive little or no education, their scores cease to be closely correlated with children of their own age groups and come to correlate more strongly with those of their parents. It seems reasonable to conclude from the examination of these and

1. Shapiro, *Migration and Environment*, Oxford, 1939.

other correlation coefficients that only about half the variance of human intelligence is due to genetical causes.[1] Other environmental factors which appear to have an adverse effect on intelligence are disease and malnutrition. Thus, the physical weakness associated with chronic hookworm infection is said to have a definitely deleterious effect on mental functioning.[2]

With regard to mental disorder, the intimate interaction of hereditary and environmental factors is also evident. Sometimes the effect of one tends to predominate. The data of E. Slater indicate that chronic depressive mania is 25 times as common among the brothers and sisters of affected individuals as it is in the general population. We can therefore infer that the hereditary influence is especially important. On the other hand, the incidence of neurosis in industry in closely correlated with particular working conditions, e.g. long hours and frequent changes of work.[3]

The separation of hereditary and environmental influences upon such complex characters presents a very difficult problem. In 1875 Galton drew attention to the value of twin studies from this point of view. The principle is sound enough. Since each member of a pair of monovular or identical twins has exactly the same genetical equipment, it can be inferred that any dissimilarity between them which is manifest must be due to different environmental agencies. This generalization requires certain reservations, however. Some developmental processes are assymetrical. Just as the two sides of a single body are unlike although they carry the same genes, so one monovular twin can differ naturally from the other in spite of having the same genetic constitution. In extreme cases, one of a pair can develop normally, while the other may remain a collection of rudimentary parts. The comparison of monovular twins with binovular, on the assumption that for binovular twins the environment is the same as for monovular, is also misleading. The postnatal environment is usually more dissimilar for binovular than for monovular twins, even where the former are of the same sex.

We must therefore interpret the results of twin studies with caution. At first sight, they seem extremely impressive and convincing. Not only are the physique and intelligence of one-egg twins closely related as a rule, but in the details of behaviour they show close similarities. Several reports of the criminal records of twins have been published. They suggest that in most instances monovular twins agree in having criminal records of the same type, whereas the records of binovular twins are little more closely correlated than those of ordinary brothers and sisters. Whenever monovular twins behave differently, external causes—such as a birth or head injury, or some disease—have been adduced as an explanation. But two points have to be recognized in this connexion. First, the coincidences of behaviour in identical twins will tend to be noticed and reported while the differences are forgotten. And, second, a high proportion of individuals in the general population will on questioning give a history of birth or head injuries or disease. There is, in fact, evidence that when identical twins are reared apart, differences develop between them. Thus Newman, Freeman and Holzinger in their book[4] report that they sometimes found marked differences in intellectual

1. Cf. Penrose, *The Biology of Mental Defect*, Sidgwick and Jackson, London, 1949.
2. *J. Educ. Psychol.*, 1946, vol. 17, p. 314.
3. Cf. Wyatt, *A Study of Certified Sickness Absence among Women in Industry*, Industrial Health Research Board, Report No. 86.
4. *Twins: A Study of Heredity and Environment*, Chicago, 1937.

ability which were closely correlated with differences in educational opportunity. Marked personality differences between one-egg twins were also observed. These authors conclude that 'physical characteristics are least affected by the environment, intelligence is affected more; educational achievement still more; and personality or temperament . . . the most'. Others have expressed similar opinions. Kallman in his book[1] states that the average difference between one-egg twin partners is no precise measure of environmentally produced variations; nor does the greater difference between two-egg twins represent the exact contribution of genetic influences even in relatively comparable environments.

It is of course true that in many instances the individual selects, and therefore determines in part, some important aspects of his environment. But it can only rarely be said that a man's behaviour is mainly determined by his genetic constitution, as Darlington implies. This statement is true of some individuals who are unable to oxidize the amino-acid phenylalanine and, because of this genetically determined biochemical abnormality, are severely defective mentally. It may be true of certain types of habitual criminal, although here the significance of environmental influences is difficult to assess. There are very few geneticists and psychologists who would go the whole way and accept genetic determinism in place of individual responsibility for criminal acts. This does not imply that most psychologists are libertarians when it comes to the problem of free will. They are thoroughly deterministic; but they recognize that, when exercising the intellectual faculties concerned with conscious choice, humans are subject to far more, and far more complex, influences than are manifest during simple reflex and conditioned reflex activity. Many of the complex influences upon conscious choice are environmental. The fact that some act is disapproved of in a particular society may well be a stronger deterrent from committing it than are the genotypes of the individual citizens. And there is no doubt that primitive and corrective measures can be effective in preventing the repetition of crimes. Darlington is right in insisting that the Old Testament notions of punishment, one as vengeance on the individual, the other as retribution by heredity to the third and fourth generation, are both indefensible. The effects of punishment on the behaviour of the individual must be examined carefully and without any sort of prejudice. Even when this is done, there are many who would disagree with Darlington when he states that the evidence

> indicates that deterrence and reformation are of strictly limited scope. And where deterrence fails and revenge is dropped the distinction, always an arbitrary distinction, between sanity and insanity no longer comes into question. For the insane felon, if left at large, is at least as likely to repeat his felony as the sane felon. Neither has any choice in the matter. . . . What courses are then left open to us? There are three: painless extinction, segregation and the prevention of birth.

We would need to be far more certain that the mainsprings of criminal behaviour are genetically determined and uncorrectable before embarking upon such radical measures.

Darlington pauses to examine the red herring loosed by Eddington and others who have suggested that there is a fundamental similarity between

1. Kallman, *Heredity in Health and Mental Disorder*, Norton, New York, 1953.

indeterminacy in physics and free will. This argument seems to be profoundly mistaken. The present failure of physicists to know why atoms sometimes behave in one way and sometimes in another is not different from many another case in the past history of science when momentary failure led individuals who had a firm grasp of the causal principle to some of the greatest triumphs of science. On the other hand, it is true that indeterminacy at the atomic level is compatible with regularity at higher levels. And there is a valid analogy between this system and the intercalated uncertainty in living systems (which is the consequence of mutation and recombination) as a means of establishing a deterministic relationship between the succession of living systems and the environment. As Darlington points out, this intercalation of uncertainty is the key mechanism in evolution.

In view of Darlington's stern belief in genetic determinism, it is not surprising that he adopts an uncompromising position with regard to innate racial differences. Referring to the advantage of the white-plus-Negro society of the southern United States over a pure Negro society and over the previously displaced Indian society he writes:

> The American Indian has been found not to be able and willing to help the other two. Their capacity and his capacity are both, of course, racial and genetic. They are determined and limited by heredity. . . . Thus we arrive at a paradox which timidity usually hides from us. The assumption of a genetic basis for race and class differences provides the evidence, the only scientific evidence, in favour of racial tolerance and co-operation.

And, again, discussing the need for mutual help between genetically diverse groups, whether races or classes, he writes that this habit cannot be 'assisted in the long run by make-believe of any kind, certainly not by a make-believe of equality in the physical, intellectual and cultural capacities of such groups'.

Statements of this kind, made authoritatively by an eminent geneticist, may well have harmful social consequences. It is a surprisingly short step to the description of the American scene in *Mein Kampf:*

> In North America, where the population is prevalently Teutonic and where those elements intermingled with the inferior races to only a very small degree, we have a quality of mankind and a civilization which are different from those in Central and South America. . . . The Teutonic element . . . has come to dominate the American continent and will remain master of it as long as that element does not fall a victim to the habit of adulterating its blood.

It is an equally short step from genetic determinism to the policies advocated by certain other racialists, e.g. the South African Nationalists who speak of the need for mutual self-help between the white and coloured populations in that country, the coloured population working as a labour force, the white minority as bearers of industrial and agricultural responsibility and, of course, culture.

That the several races of mankind differ in certain genetically determined physical characters (e.g. skin colour and hair form) no one would attempt to deny. That races differ in cultural and intellectual achievements is also obvious, but whether these latter differences are due to innate or circum-

stantial factors is one of the most disputed subjects in anthropology. Because of its social and political implications this controversy has tended to generate violent emotions which are unfavourable to clear thought. If there is ever to be agreement about the problem of inherent racial differences, it will not emerge from the type of subjective theorizing which has been widespread up to now. Some of the evidence regarding inherent racial differences in intelligence will now be examined briefly.

Let it be said at once that the concept of general intelligence tends to become strained when people of very different cultural backgrounds are compared. None of the tests so far devised measures innate or genetically determined ability regardless of the effects of environment. Moreover, there are serious difficulties in the application of intelligence tests to different groups, among them variability in the motivation to succeed in the tests, in the relation between experimenter and subject, in the familiarity with the language used and in the ease of communication on the part of the subject. There is also the difficulty of sampling so as to test representative populations. Nevertheless, and in spite of these difficulties, the results of the tests are objective and worthy of consideration in the midst of a plethora of subjective value judgments.

The traditional idea of separating mankind into constituent races, each endowed with a particular set of mental qualities, has received little support from the results of the tests. Thus, an analysis of intelligence tests on United States army personnel showed that the mean scores of whites were somewhat superior to those of Negroes, although there was a considerable overlap between scores. However, Negroes from northern states, such as New York and Ohio, were superior to whites from the southern states of Georgia, Arkansas, Kentucky and Mississippi.[1] Even where the cultural background is relatively homogeneous, as in Jamaica, the results have been equivocal. In one series of tests the coloured children were found to be superior in mental arithmetic and tasks related to musical ability, whereas the white group was superior in most (but not all) verbal tests and in general knowledge. There seems to be no lack of very superior Negro children in public high schools in the United States.[2] Numerous other tests of this kind are quoted by Klineberg in his book *Race Differences*.[3] In view of all these negative results it seems highly improbable that there will ever be found innate differences between American Negroes and whites so large that they could justify a differential treatment in matters of public policy such as education, suffrage and entrance to various sections of the labour market.

Darlington writes, moreover, when describing the apparent backwardness of the Australian 'black-fellows', that 'it is supposed to be due largely to their inadequate heredity'. This statement begs the fundamental questions. Is the behaviour of such populations in fact conditioned by their innate capabilities or by the environment in which they happen to be? And, even supposing that the difference is largely hereditary, with reference to what is it inadequate? The particular criteria of intelligence and ability developed in Western European urban society may not be applicable to groups with different backgrounds. The relativity of the concept of intelligence is shown when tests

1. *J. Soc. Psychol.*, 1945, vol. 40, p. 344.
2. *J. Educ. Psychol.*, 1934, vol. 25, p. 585.
3. Klineberg, *Race Differences*, Harper, New York, 1935.

are devised which are based upon different criteria from our own. When Porteus tested the Australian natives for the ability to match photographs of footprints, in spite of their unfamiliarity with the test situation, they did just as well as the group of presumably superior white students in Hawaii. The designation 'primitive' has been applied to the Australian aboriginal population mainly because its economy is the 'simplest' known to man: that of hunting and food-gathering. But whether the extraordinary imaginative and symbolic powers shown in their elaborate system of totemism are indicative of equally 'primitive' and innately limited minds is, to say the least, doubtful. Recently the Australian natives have shown themselves capable of expressing ideas forcefully and imaginatively in paintings. It is a fallacy to suppose that because certain people have no tradition in one or more of the arts or professions they are innately incapable. This was strikingly illustrated by an exhibition of paintings from the Cyrene Mission in Rhodesia held recently at the Central School of Art in London. The Africans in the area concerned have no tradition in the plastic arts; yet, when given modern materials, they were able to express their observations and ideas freely in paintings of sufficiently high quality to draw praise from critics who had no axe to grind.

As far as contributions to past civilizations are concerned, no substantial evidence has been produced to contradict the conclusion of Toynbee: 'The upshot of our enquiry is to discredit the hypothesis of a natural law in which the creation of civilizations is supposedly revealed as the peculiar racial function of particular branches of the human family.'[1]

As it is with races, so it is with classes in society. It is of course true that the managerial and executive classes are, in general, more able intellectually than the manual labourers. But the measure of permanence that is given to classes is the outcome of environmental as well as hereditary characteristics. Enough has been said of the importance of environment to show that it is unnecessary to accept with Darlington that classes of men are firmly differentiated genetically and that Plato's 'noble lie' is a description of human society as it has always been. In the *Republic* Plato recommended that the best men should mate with the best women in order to maintain a stable society. But where hereditary privilege ceases to be all-important, as in the United States, the class barriers have effectively broken down and there is a great deal of interchange of individuals between them.

Darlington concludes his book with an interpretation of history founded on biological determinism. It is a 'great man' theory of history; and the individual men are great, of course, because of their heredity.

> If the part played by individuals in the history of war, or government, or science, does not convince the reader, let him reflect a moment on the history of art. . . . Did Michelangelo and Johann Sebastian Bach respond to their environments? On the contrary, by deliberate self-assertion they made their environments respond to them. . . . The part played by individuals in the history of man fills every moment of our lives.

It is easy enough to reply that the social context plays a powerful part in moulding an individual's reactions, and in creating opportunities for great achievement. The frequency with which parallel discoveries have occurred

1. Toynbee, *A Study of History*, Oxford, 1934, vol. 1, p. 429.

in the history of science should convince us that we would not be without a general theory of gravitation had Newton died as a child. Seventeenth-century Dutch painting would be great even if Rembrandt had never been born.

But when it comes to historical interpretation, and in the general problem of genetic determinism, the truth of the matter seems to lie half-way towards Darlington's point of view. Who can deny that 'taking all possible circumstances into account and allowing for an infinite variety of circumstances, there are certain limits to what the product of a particular egg can do, and these limits are laid down by the chemical and physical structure of that egg'? The only problem is how narrowly we define those limits. And here it seems that Darlington is inclined to overstate his case. There is no need to do this: the facts are formidable enough. In the long run this determinism rests on the greatest of all chances: the genetical constitution of the particular spermatozoon and ovum that unite to form an individual. The very next spermatozoon to the one that fertilized the egg which came to be Newton might have produced an unknown woman—or a monster. Each ejaculum of the human male contains something like a thousand million spermatozoa, most of which are capable of fertilization. In this material there is tremendous potential variability. But once the union with the egg is achieved the limits are imposed. Darlington correctly states that this 'constitutes, as timid souls will quickly declare, a sentence of predestination. But to bolder minds it is also the charter of individuality'.

There is an interesting parallel to be recognized between the consequences of this type of predestination and the predestination of the Calvinists. Even though Calvin taught that God foreknew and predestined the fate of every man from the beginning, this did not mean that his followers must abandon themselves with fatalism to this grand decree. On the contrary, they were expected to behave as befitted 'the elect', to demonstrate their moral superiority by right living. It was with this confidence, and in this tradition, that the Huguenots of France, the Puritans of England and New England, and the Scottish Calvinists played their important part in history. So it should be with genetic determinism, which is biological predestination. The fact that an individual has inherited superior faculties, that he is one of the elect, imposes upon him a responsibility to use his faculties wisely and honourably. Perhaps he should, like another Puritan, 'scorn delights and live laborious days'. *Noblesse oblige.*

The Facts of Life contains a great deal of valuable information. It is written extremely clearly and in carefully chosen language. Many of the generalizations are brilliant and penetrating. There are aphorisms on every page. But in the opinion of the reviewer it is not good popular science. It goes so far beyond what is scientifically established that it may well give the public a distorted view of current genetical thought. The social implications of Darlington's statements have to be hedged in with numerous reservations which the general reader must make for himself. The book is, however, a powerful stimulus to thought throughout, and that alone is no small achievement.

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